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MEMORIES OF A SCIENTIFIC LIFE

MEMORIES OF A SCIENTIFIC LIFE

BY

SIR AMBROSE FLEMING

M.A., D.Sc., F.R.S.

MERITUS PROFESSOR OF ELECTRICAL ENGINEERING IN THE UNIVERSITY OF LONDON;
HONORARY FELLOW OF ST JOHN'S COLLEGE, CAMBRIDGE;
FELLOW OF UNIVERSITY COLLEGE, LONDON; HONORARY MEMBER AND
FARADAY MEDALLIST OF THE INSTITUTION OF ELECTRICAL ENGINEERS, LONDON;
PRESIDENT OF THE VICTORIA INSTITUTE; PRESIDENT OF THE TELEVISION SOCIETY;
HONORARY FELLOW OF THE INSTITUTE OF PHYSICS,
ALBERT MEDALLIST OF THE ROYAL SOCIETY OF ARTS,
ETC ETC.

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TO
MY DEAR WIFE

FOREWORD

by

SIR OLIVER LODGE, K.T.,
F.R.S., D.Sc., LL.D., M.INST.E.E.

A Letter to the Author

MY DEAR FLEMING,—

I have to-day read the proof of your *Memories* with the greatest interest, and I am struck with the somewhat similar experiences and opportunities that we have both had, though we made different use of them. Sixty-two years ago I made your acquaintance in Professor Frankland's Advanced Chemical Laboratory under Valentin; and I remember how you took me once to your rooms and we discussed many physical topics. We discussed the researches of Sir William Crookes on the apparent pressure of light on a bar of pith, that led up to his invention of the radiometer. And then the seat of the E.M.F. in a voltaic battery—a battery which undoubtedly worked without metallic contact, and constituted the germ of your first paper to the Physical Society. We were both more interested in Physics than in the Chemistry to which we then had to devote our days. I especially remember your admiration of Clerk Maxwell and his brilliant discovery of the ratio “ v ,” and our joint wish to go to Cambridge to be under him—a wish which you presently translated into action.

After this winter we separated; you went to Cam-

bridge, obtaining a scholarship at St. John's College, while I migrated as a belated student to University College, London. Both suffered from the prevailing ignorance and lack of interest in everything scientific. You were the more venturesome. I am tempted to trace analogies in all the events of our lives. In 1881 we were appointed to Chairs at Nottingham and Liverpool. Then both had an opportunity of going to London, which you took, while I didn't. You went as a Professor of Engineering, through a great period of development both of telephone and electric light, to become the scientific and engineering adviser of Marconi, and gave him the great advantage of your help at the Poldhu Station. At length your researches, communicated to the Physical Society, led you to the high-frequency vacuum rectifier, which you applied as valve detector of ether waves, and which in somewhat improved form has rendered possible all the broadcasting of speech and music by continuous waves which now goes on all over the world.

We both received a Medal from the Royal Society and the Faraday Medal from the Electrical Engineers, both the Albert Medal from the Duke of Connaught as President of the Society of Arts. And now having both passed the fourscore milestone we await a peaceful ending to our troublesome but interesting earth experiences, the one in Devonshire, the other in Wiltshire, in full trust and confidence that our faults will be mercifully regarded and our efforts appreciated.

Yours sincerely,

(Signed) OLIVER LODGE.

24th July 1934.

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MEMORIES OF A SCIENTIFIC LIFE

CHAPTER I A JUVENILE SCIENTIST

THE question is often asked of elderly persons, What is the earliest event in your life you remember, and how old were you then? No baby remembers being born, nor perhaps has any clear recollection of events in the first two years of its life. It is perfectly conscious of pleasure or pain, but definite memory of events probably does not begin until after the end of the second year of life, or even later.

The writer of these memories was born at Lancaster on November 29, 1849, in a house called "Greenfield," on the outskirts of the town.

His earliest recollection is a little escapade with a bolt on the nursery door. His nurse had gone down to fetch the breakfast and closed the door. There was a bolt on the inside; and probably without any intention of mischief but merely from baby curiosity the young investigator

slid the bolt forward. When the nurse came up she could not open the door, and called repeatedly to the little prisoner to open it. But whether from fear or ignorance he could not do it. So finally his father had to be called, and the bottom panel of the door kicked in so that he could put his hand in, and having withdrawn the bolt could admit the nurse.

At the date of this event the writer was probably between two and three years old. When he was about five years old, his father and mother removed their residence to the north of London. His mother's father was Mr. John Bazley White, one of the pioneers in the manufacture of Portland cement, and he had established a large factory for this work at Greenhithe, in Kent, and built himself a private residence, entirely of concrete, on ground at Swanscombe, adjacent to his works. Accordingly, even before the removal to London, there were occasional visits to this maternal grandfather's house, and memories of long railway journeys from Lancaster to London and thence to Swanscombe.

The building material called "Portland cement," which is made by mixing together chalk and clay and firing it in a kiln, had that name given to it by Joseph Aspdin, a bricklayer of Leeds. A large number of inventors

from the time of the Romans downwards had some share in the discovery of this valuable material. When ground to fine powder and mixed with water it has the property of setting hard even under water. A book by Mr. A. C. Davis gives the history of it for the last hundred years. It can best be manufactured at places where clay and chalk occur near each other, as is the case near Greenhithe, in Kent, England, where my grandfather settled, and to his house I often went as a child.

These railway travels from Lancaster to London excited in my infant mind an intense interest in engines of all kinds, and especially locomotives and all the appliances of railway working, and I very soon began with pencil and paper to make childish drawings of a locomotive. These were then mostly of six-wheel type, the centre pair or driving wheels being larger than the smaller front and back pair. I remember I borrowed a penny to enable me to draw the circles for the big wheel and a half-penny or farthing for the smaller ones. Later on, I had given me a large coloured drawing, showing a section of the locomotive, and became well acquainted with its internal "works." I was always intensely interested when taken to see the stationary engines in my grandfather's cement works and in all the other machinery

of the place. In fact, I early exhibited what is called a mechanical turn of mind and interest in things rather than persons.

How this was acquired I cannot say, because only one other member of a large family developed any scientific tastes, and the rest were quite indifferent to such tastes. Nevertheless, on my mother's side my relatives were all keenly intelligent, and two or three became well known to the general public, namely, her sister, Mrs. Ellen Ranyard, who founded the Ranyard Biblewomen's Mission, and her brother, the Rev. Edward White, an author as well as an eloquent and instructive preacher.

My grandfather had nine children, five sons and four daughters. Three of his sons succeeded to his business and became rich men. Two of the daughters married fairly well-to-do men, and the rest of us had to make our way as best we could in the world.

When my parents came to London, they lived in a house in Rosebery Villas, north of Kentish Town, and as other children were born removed to a larger house in Tufnell Park, near by. At that time the northern suburbs of London were sparsely populated, and from behind the road in which we lived grass fields, golden or silver in summer with buttercups and daisies, stretched for miles up to Highgate and Holloway. Part

of it formed a cricket ground, where well-known players were seen, Dr. W. G. Grace amongst the number.

My earliest recollection of the life at Rosebery Villas was being taught to read by our mother—I and my second brother, Howard, who was two years younger, being seated on two footstools at our mother's knee. Also I remember my brother and myself having our portraits painted by a lady artist. In those days childish education consisted chiefly in "learning by heart," as it was called, or committing to memory, verses from the Bible, poems, or hymns, or answers to Catechism questions, and the multiplication table. A book I remember was called a *Child's Guide to Knowledge*, and the first question in it was, "What are the three diseases of wheat?" The answer was, "Smut, blight, and mildew." Very useful information for a farmer, but hardly so for a child.

As soon as we could read and walk alone, my brother and I were sent to a ladies' school in Kentish Town, kept by a Miss Hill, and there the process of "learning by heart" was continued. We had also lessons in what was called "deportment," or a rudimentary drill. Religious education was also a feature. Children in those days were more strictly treated than at present.

They were not encouraged to talk much or frivolously, and were often told they must be "seen and not heard." Their childish questions were not always given satisfying replies, and I remember that when I asked an elderly relative the cause of thunder and lightning, I was told it was due to "two clouds knocking together." Such was the appalling ignorance of our elders in those days.

When I was about nine years old, in 1858, a large comet (Donati's) made its appearance in the sky. At its brightest the tail stretched as a luminous arch a quarter of the way across the night sky. Although this celestial visitor caused me a certain amount of alarm as a possible portent of disaster, it led me to take a great interest in astronomy. I found a book by Mrs. Somerville in the house, and an early edition of an encyclopædia which gave me information.

At about ten or eleven years of age some one gave me a box of magnetic toys. It contained a horseshoe steel magnet and some bar magnets. I particularly remember a small model of a swan which could be floated on water, and contained a bit of iron in its interior, so that it could be guided about by the attraction of a magnet. All these things, model ships and engines, interested me enormously. A box of tools by which I could make things myself out

of bits of wood, odds and ends collected about the house, was given to me, and variously employed.

When I was about twelve years of age my brother and I were sent to a school, kept by a Mr. de Lancy, which was near at hand, in a road called Hilldrop Crescent. He taught us Geometry in a practical manner with drawing instruments, and I soon began to make some little progress in the study of Euclid, which was the text-book in those days. Meanwhile, I steadily followed my own handicraft pursuits at home. I carved out and rigged quite a large model yacht, which I used to sail on the Hampstead Ponds.

At that time amateur photography began to be practised. There were no dry plates. The amateur had to coat his own glass plates with collodion, sensitize them by immersion in a bath of silver nitrate, and then develop and fix with cyanide of potassium. I remember making a rudimentary camera with a cigar box and a magnifying-glass, and acquired in some way a stock of the required chemicals. My first attempt was to photograph a large white jug with a black cloth behind it. When I discovered there was an image on the plate, I rushed off in delight to show it to my mother, but omitted in my hurry to "fix" it; and hence, when I hoped

to exhibit my achievement, the plate was all black and there was nothing to show.

One of my great joys at that time was to be taken by my dear mother to the "Polytechnic," in Regent Street, London. This was an institution founded about the middle of the last century to interest the public in the mysteries of science. The presiding spirit of the place was then Professor Pepper, who had invented an illusion called "Pepper's Ghost," in which a ghost appears and disappears suddenly in orthodox fashion in a scene enacted in a supposed haunted house.

The illusion was managed in the following way: The whole front of the small stage was filled in with a single sheet of plate glass inclined slightly forward at the top. In the front of the stage, in a position concealed from the audience, was a man dressed up in a white sheet to represent the ghost. He was brilliantly illuminated by light from an oxyhydrogen lantern. The audience saw his reflection on the glass sheet, and also saw through the glass sheet to the stage equipment beyond. When the illumination of the man was turned off, the ghost vanished. Hence it could be made to appear or disappear at pleasure in various scenes, such as appearing at the bottom of a four-post bed and alarming the occupant.

This "Polytechnic" contained a large hall surrounded by galleries and lecture theatres adjoining, in which were exhibited "dissolving views," shown with a pair of oxyhydrogen light magic-lanterns.

Just before that time the Indian Sepoy Mutiny had taken place, about 1857, and I have a clear remembrance of the thrilling pictures of the siege of Lucknow and other Indian Mutiny scenes. In the central hall was a large deep tank of water, into which at certain times a man in diving dress went down, and picked up pennies thrown to him into the water, and tapped them on his metal helmet to prove he had them. Over the tank was suspended by a crane a diving-bell, and visitors, by paying sixpence or a shilling, could experience the thrill of being let down therein into the tank. One day an American visitor got into the bell, and it was being moved off over the tank for a descent; whereupon he shouted out, "Stop! Stop!" When the bell was pulled back, and he was asked what was the matter, he exclaimed, "I guess I am not going down in this thing. It has got no bottom to it!"

There were also lectures on Chemistry at various times. I remember one exciting experiment on a method of rendering linen or cotton fabric non-inflammable. In those days girls

wore white muslin or other dainty material for their party frocks, and these were dangerously inflammable. If, however, the fabric was prepared by being dipped in a certain solution of sodic paratungstate, a compound of soda and tungstic acid, the mysterious chemical formula for which is $5\text{Na}_2\text{O} \cdot 12\text{WO}_3 \cdot 28\text{H}_2\text{O}$, and then well dried, it became non-inflammable.

This, I remember, was illustrated by a striking experiment. A young lady-assistant was brought into the lecture-room arrayed in a white muslin dress with the ample skirts of those days (about 1860-62), which dress had been rendered by the above treatment non-inflammable. The lecturer ran up and down her dress a lighted taper in a way which would have caused instant disaster had not the dress been prepared, but in this case the skirts did not take fire. Then a large doll similarly dressed, but with the skirts not prepared, was put on the stage, and the lighted taper applied. Instantly the muslin dress caught fire, and formed a pyramid of flame in which a real girl would have perished miserably. The moral instilled was that party frocks should be laundered with a solution of sodic paratungstate, but I do not think that the precaution was often carried out.

Professor Pepper was also the author of a book called *The Play-Book of Science*, in which a large

amount of interesting information was contained. It was a much-read volume in my private boyish library.

When I was fourteen years old my parents decided that I should be sent to University College School, which was a day school conducted in a wing of University College, in Gower Street, London. The College was founded in the year 1827, to provide higher education for those who could not attend or gain entrance to Oxford or Cambridge, then only open to members of the Church of England. The school was founded as an appendage to the college. The headmaster in my time was Thomas Hewitt Key, who was a great philologist or student of language, as well as an impressive personality.

Gower Street was then the place of residence of many well-known professional men, and Charles Darwin, the great naturalist, resided from 1839 to 1842 at No. 112 Gower Street, which is now part of a hostel for Indian students. Many of the boys attending University College School became well known in after life. Sir Herbert Samuel, the present Leader of the Liberal Party, and W. W. R. Ball, a late eminent mathematician and tutor of Trinity College, Cambridge, were contemporaries with the author of this book.

It was the boast of the school that it was conducted without corporal punishment, which was considered to be in those days a singular omission, as in the older public schools boys were thrashed by headmasters with frequency and vigour. Key had a custom that any boy who was forward enough to be in the sixth mathematics class (the highest) should also be in the sixth classics. I had been well taught in mathematics, and had a taste for it, but was an extremely poor classical scholar. My usual place then in the Latin class, conducted by Key himself, was at the bottom. But Key was accustomed to put questions to test his boy's general knowledge, and one day, by a lucky answer, I got to the top of the class, and was put on to construe. The result was a disastrous failure, and Key, in a gently sarcastic voice, recommended me to take my usual place at the bottom of the class.

About that time, however, my parents began to consider the important question of my future work. My mother had a serious conversation with me. With my love of machinery I had hoped to be an engineer, and perhaps, if I was good, to be allowed to drive an engine! But there were no Engineering Colleges in those days, and the only way in which a boy could become a mechanical engineer was by being

articled to an engineering firm. But this involved paying a premium, often very large, for gentlemen apprentices. This my father with his young family could not afford. Accordingly, I was told that as a first step I must prepare myself for the London Matriculation examination. The University of London at that time was a purely degree-giving body, which conducted examinations and awarded degrees, but did no teaching. The "Matric. exam." as it was briefly called, involved being prepared to be examined in certain Greek and Latin pre-notified books, and in Mathematics and Elementary Science. In my case I had to prepare a book in Homer's *Iliad* and an oration (Latin) of Cicero. As this lay outside my school work, I remember I had to get up early in the morning and fag at it in my bedroom, and my father gave me some help after breakfast.

As Gower Street was about two or three miles from Tufnell Park where we lived, the school attendance involved a long walk there and back each day, and we were provided with a sandwich for lunch at the midday interval. However, I managed to pass the Matriculation examination when I was a little over sixteen years of age, and was then entered as a student in the College, to prepare for a Science degree.

Before describing my College life, mention

may be made of the manner in which my attention was drawn to the Science of Electricity, which has been the chief subject of my life study. My father had a friend, Mr. Dixon, who was an architect by profession and an amateur scientist by choice. When I was somewhere between twelve and fourteen years old, Mr. Dixon gave a lecture on Electricity at some school in Kentish Town, and showed experiments which attracted my interest enormously. He lit a candle and fired a small cannon by electricity, and explained the mode of making a voltaic battery for producing the electric current, and of a frictional electrical machine for generating charges of electricity. Thereupon I resolved to repeat these interesting experiments for myself.

All available pocket money was therefore expended in the purchase of a dozen jam pots, some zinc and copper plates, and a supply of sulphuric acid for the manufacture of a voltaic battery. But in my ignorance, I made the acid solution in the pots too strong, and in a short time the zinc plates put in them all dissolved away. I then told my troubles to Mr. Dixon, and he explained to me that the zinc plates must be *amalgamated* by rubbing them with mercury or quicksilver, and also that the acid must be diluted with ten times its bulk of water, and that then the zinc plates would not dissolve

away uselessly, but only when the battery was creating an electric current.

Provided with these valuable hints, I attacked my job once more. Each pot contained a plate of amalgamated zinc and a copper plate not touching it. Then the zinc in one pot was joined by a short wire to the copper of the next pot, and so on for the series. The copper in the pot at one end had a wire attached to it, and similarly for the zinc at the other-end pot. When these two end wires were made to touch so as to complete the circuit, an electric current flowed through it. If these terminal wires were closely covered with cotton thread to *insulate* them, and then wound many times round an iron bar such as a poker, the current caused the bar to become a magnet as long as the circuit was complete.

I constructed such an electro-magnet by having a stout short bar of iron bent into a horseshoe or hairpin shape by a blacksmith, and then I wound over the bar with many turns of copper wire which had been closely spun over with cotton thread to prevent the convolutions of copper wire touching each other. When this wire had its ends connected to the terminal wires of the battery, it became a powerful magnet and would hold up a heavy weight when attached to a piece of iron held against the ends or poles

of the iron horseshoe. This important invention of the electro-magnet was made by William Sturgeon in 1825, who was a shoemaker by trade, and afterwards became a private soldier in the Royal Artillery. It was improved by Joseph Henry in America, and is an essential element in every electric bell and telegraphic instrument.

At a little later date I made a frictional electrical machine. I obtained in some way a glass cylinder, and mounted it in a support so that it could be turned round like a grindstone by a handle. Against this was pressed a cushion covered with silk, and, when revolved so that the cushion pressed steadily against the glass cylinder, this latter became highly electrified. I made also a Leyden jar from a large empty pickle bottle, which I lined inside and outside with tinfoil, pasted on. The Leyden jar could then be charged with electricity by the electrical machine ; and it gave powerful electric shocks if both the outside tinfoil and the inner were touched simultaneously with the two hands. I was always pleased to administer a shock to any one, especially those who doubted the efficiency of my electrical machine.

Later, I became possessed of sufficient funds to buy three cells, of a battery called Smee's. Each glass cell contained a silver plate, having on it platinum black, and each side a thick amalgam-

ated zinc plate. I arranged my cells in a box, so that the plates could be let down into the acid or drawn up when not in use, and I had thus the means for providing myself with electric charges or electric currents at pleasure, and to show various remarkable experiments.

Another among my father's friends who helped my practical knowledge of electricity was a gentleman who was a telegraphic engineer, Mr. Bartholomew. Before 1870 electric telegraphy in Great Britain was conducted by public companies, but was not State-controlled. Mr. Bartholomew was a clever inventor of telegraphic apparatus, and he gave me my first lesson in making an induction coil for creating electric sparks. Later on, I constructed a larger coil, consisting of a bundle of iron wires wound over with a thick insulated copper wire called the primary circuit, and a great length of many hundred turns of silk-covered fine copper wire called the secondary circuit. When an intermittent electric current was sent through the primary, electric sparks an inch long were obtained from the secondary circuit.

It seemed to me then that the time had come for me to launch out into the career of a juvenile science lecturer, and to share my knowledge with my family. Accordingly, I set up a lecture table, formed of a spare leaf of a mahogany

dining-table placed across the backs of two chairs in a corner of the family sitting-room, and on it placed my pieces of apparatus. My brother, Howard, arranged chairs in front of it, and cut up some cards to form tickets for what he called *preserved* seats. My audience was small but quite appreciative, and the experiments all a success, particularly that of making them all join hands and bidding the one at one end of the chain to touch with one knuckle the outside of my charged Leyden jar, whilst the other at the opposite end would touch the knob. An electric shock then ran through the family circle, and the audience broke up with shouts of laughter.

Returning then to my College experience at University College, London, I came at once under the influence of several well-known scientific men. In Mathematics, Augustus de Morgan was the Professor, a man of vast erudition, and great also as a teacher. In everything he demanded the most exhaustive proof and thoroughness. Among his other works he had written a treatise on the *Differential and Integral Calculus*. It was a big book, printed mostly in very small type, and it was a common tradition among his students that only two persons had read every line of it, namely, de Morgan himself and the printer.

He was a large man, and usually dressed in a black cloth coat, cut like a modern swallow-tail dress-coat, and also with a voluminous white necktie. If any of his students were inattentive at his lectures he used to say in a solemn voice, "I think there are some gentlemen here who have not quite grown up to their age." Nevertheless, to those who did pay attention, he was an influence that lasted for life.

Then in Chemistry there was Professor Alexander Williamson, and in Physics, Professor Carey Foster, both of them men who made their mark on their age.

My object then was to obtain the degree of Bachelor of Science (B.Sc.) in the University of London. It had to be taken in two parts, and required some knowledge of a number of subjects. The first examination was called the First B.Sc., and in addition to Pure and Applied Mathematics, Chemistry, and Physics, the candidate had to know a good deal of Botany (about flowers) and Zoology (about animals), which were subjects I had never touched. Accordingly, I was not able to pass at the end of my first College session, but did so at the end of the second year.

The question then became a pressing one as to future occupation. An opportunity presented itself of some position in a firm of ship-

builders near Dublin, in Ireland, Messrs. Walpole, Webb, & Bewley, with some small salary, and I was urged to take it. That was my first experience of life away from the comforts of home. I had to live alone in lodgings, and go day by day to the shipyard. But I found that the work I was set to do was merely copying plans of ships in the drawing office, with no opportunity of learning practical engineering work in the shops. So after three or four months I begged my parents to let me return home and continue to work for my second B.Sc. exam., and obtain the degree. This they agreed to do, but as it was necessary for me at that age to do a little remunerative work, a position was found for me as a clerk on the Stock Exchange, where the hours of work were short. My parents had at that time removed to a house in the Camden Road, and there was a railway station close by which enabled me to reach the Moorgate Street Terminus in a few minutes.

As the work on the Stock Exchange did not begin until 10 or 10.30 a.m. and was closed soon after 4 p.m., and it was possible to go and come in a short time from my home, I had a good deal of time for private study. Before long I became acquainted with all the mysteries of buying and selling stocks and shares and the special aims of the "Bulls," "Bears," and

“Stags” of the Stock Exchange in their efforts to make money out of the trustful and optimistic public.

I had, therefore, plenty of time in morning and evening for work for my examination. The second B.Sc. required a more extensive knowledge of the subjects set at the first examination, and several fresh ones in addition, such as Geology, Physiology, Zoology, and Mental Philosophy. It was in scope much broader and less highly specialized than it is to-day. It is, however, a great advantage to know a little of everything and everything of something, and the fault of the degree syllabus at present is that the candidate has to be highly acquainted with some few subjects and may be entirely ignorant of many others. The experience I gained also, by about two years' work in the City Office, gave me a knowledge of business and of matters of investment which has been of great advantage in later life. Every boy and girl should have some practical training in these matters.

By very hard work, however, in early morning and in evening at home, I acquired the requisite amount of knowledge for the final B.Sc. exam., and sat for it in the autumn of 1870. I and one other candidate were the only names placed in the First Class as shown in the following list :

COPY OF THE LIST OF SUCCESSFUL CANDIDATES AT THE EXAMINATION FOR THE DEGREE OF BACHELOR OF SCIENCE, UNIVERSITY OF LONDON, 1870.

First Division

Fleming, John Ambrose	.	University College.
Hick, Thomas B. A.	.	Private Study.

Second Division

Avcling, Ed. B.	.	University College.
Clowes, Frank	.	College of Chemistry.
Dyer, W. T. Thiselton	.	Private Study.
Hartley, J. A.	.	University College.
Martin, H. Newell	.	Christ's College, Cambridge.
Moorhouse, T. F.	.	Private Study.
Roberts, Robt. D.	.	University College.
Routledge, R.	.	Owen's College.
Salter, Frank	.	University College.

As soon as this success had been obtained, and I was a B.Sc. of the University of London, I bade farewell to the City of London. My father, in his Lancaster days, had become acquainted with a young man who had risen to be one of the most eminent of the scientific chemists of the time, namely, Dr. Edward Frankland. He had studied in Germany, and was then the Professor of Chemistry in the Royal College of Chemistry in London, being a great authority on all questions of chemistry concerning national welfare. He was also known as the discoverer of a group of substances called the organo-metallic bodies. My father thereupon wrote to him, asking for an interview so that he might consult him on my future work. I had by that

time realized that it was out of the question for me to enter the engineering profession as an articled pupil, and that the only way by which I could engage in scientific pursuits and follow the bent of my mind was by preparing myself for the teaching profession, so as ultimately to obtain time and opportunity for serious and original scientific research.

At that time Dr. Frankland had been asked to recommend a Science teacher for Rossall College, a public school for boys on the Lancashire coast, between Fleetwood and Blackpool, and he accordingly advised me to prepare myself to take this appointment, which I accordingly did. The position thus offered to me carried with it a salary of £140 per annum, in addition to full board and lodging, and the advantages of six or seven weeks' holiday in the year.

Dr. Frankland, who afterwards became Sir Edward Frankland, remained always my most valued friend and supporter, and I owe a great deal to his helpful advice and kindness.

Accordingly, I made all my preparations. I was then just twenty-one years of age, no longer a juvenile, but a young man embarking on the voyage of life with high hopes and an intense interest in the work before him. So in January 1871 I began my new employment as Science Master in Rossall School.

CHAPTER II

SCIENCE MASTER AND SCIENCE STUDENT

ROSSALL COLLEGE, to which I then went on my first engagement as a Science teacher, is a public school for boys on a lonely spot on the coast of Lancashire, about four miles from Blackpool and three miles from Fleetwood. It consisted of a series of school buildings round a quadrangle, with a headmaster's house on one side and a chapel on another. Each assistant master had a sitting-room and bedroom in some part of the buildings and a classroom in which he taught. I found that there was little or no apparatus for teaching Science or Chemistry, which was the subject I had to teach, and that only about one hour a week was allotted to each class, but the whole school had to be examined at intervals on some subject, such as Elementary Geology. There was also a small astronomical observatory in the grounds. The assistant masters took their meals in a Common Room, but after school hours they were free to visit each other in their rooms, or work privately in their own.

The subject (Chemistry) I was engaged to teach is not one well adapted as an educational implement for boys, unless it can be taught to them practically. It is useless to lecture to boys as one would do to adults. They simply do not listen. The main purpose of Science teaching in schools should be to train the boys' powers of observation and inference. Hence such teaching should largely consist in performing experiments, and making the boys note what takes place. I found it to be a good plan to do one or more experiments in complete silence, and then set the boys up in a line in class, and cross-examine them as to what they had seen and what they had inferred, and let them "take places" for good answers—in other words, to make a "game" of it. There is no advantage to the boys in merely committing to memory statements told to them by word of mouth. They must be trained to put their own observations into writing, and produce good notes neatly and carefully written out, and illustrated by drawings. For older boys, when a laboratory is available, chemical analysis of inorganic mixtures is a useful training.

As far as the time permitted, I endeavoured to put these principles into practice at Rossall, and that I was not altogether unsuccessful is shown by the testimonials given me by the head-

master, Mr. Henniker, and the vice-headmaster, Mr. Phillips, when I left—as follows :

Copy of a Testimonial from the Rev. R. Henniker, M.A., late Principal of Rossall College.

ROSSALL, September 28, 1872.

I hereby certify that Mr. JOHN A. FLEMING, B.Sc. (London), has been Science Master here at Rossall upwards of a year and a half. He has during that time shown great zeal and energy in discharging his duties. His lectures, some of which I have attended myself with much interest, are elaborate works of art. I have been particularly struck by his common-sense grasp of his subjects, and his intelligent appreciation of the difficulties of science as felt by minds of less scientific education. Moral and religious character unexceptionable.

(Signed) ROBERT HENNIKER, M.A.,
Trinity College, Oxford ;
Headmaster of Rossall.

Copy of a Testimonial from the Rev. S. H. Phillips, M.A., Vice-Master of Rossall College.

Mr. J. A. FLEMING was formerly for nearly two years Science Master in this School, and left us to devote himself more assiduously to the study of Science than his duties here permitted. He had large classes to teach, and had them well in hand. Science has never been better taught here than when it was in the hands of Mr.

Fleming, and what I saw of him here gave me the impression that a man so generally intelligent (for Mr. Fleming has much general knowledge) and so devoted to Science was likely to achieve distinction. Adverting to his kindly yet firm dealing with his boys, his thorough knowledge of the subjects which he teaches, and his moral excellence, I can confidently recommend him for the position of Science Lecturer which he is seeking in Dulwich College.

May 25, 1874.

The reason I resigned my position at Rossall was because I felt that I myself needed a higher training in scientific work, and that I had in some senses more need to be still a student rather than a teacher. Accordingly, I implored my parents to let me come back to London and reside at home, and work for a year or two in Dr. Frankland's laboratory. This they kindly allowed.

At that time the old Royal College of Chemistry in Oxford Street had been closed, and a new and vastly better building equipped at South Kensington, called the Science Schools, at the end of the Exhibition Road, London, S.W.7. Dr. Frankland was established there as Professor of Chemistry ; Dr. Frederick Guthrie as Professor of Physics ; and T. H. Huxley as Biology Professor ; and later on Sir Norman Lockyer as Head of the Solar Physics Department.

I then entered the practical Chemical Laboratory for advanced students, under Dr. Frankland, and attended his lectures. One of my fellow-students in those days was the now eminent scientific man, Sir Oliver Lodge. His subsequent researches on electric oscillations and electric waves rendered him one of the great pioneers in electric wave telegraphy. The senior laboratory at the time was under the care of Mr. Valentin.

Part of the course consisted in giving to students mixtures of chemical substances which they had to analyse. After a short time there, Dr. Frankland took me into his private Research Laboratory, as one of his Research Assistants. I had as a colleague Mr. R. Meldola, who afterwards became famous as a chemist. We prepared for Dr. Frankland stores of organo-metallic substances. Mr. Meldola and I made quantities of boric ethide and zinc ethide for Dr. Frankland's experiments. This last-named substance is spontaneously inflammable, and hence must be handled with care.

I was also engaged in work on gas analysis, with an apparatus devised by Dr. Frankland and Professor Armstrong. This brought me in contact with Professor Guthrie, who became a most valued friend. Guthrie was a very ardent experimentalist, and at one time he also had been an assistant to Dr. Frankland.

He had made an experiment with two rods of coke, placing them beneath some liquid called bisulphide of carbon, and passing a strong electric current across from rod to rod, and found bubbles of a gas coming off. Guthrie asked my assistance in endeavouring to discover the chemical composition of this gas. I forget whether we were successful in our work ; at any rate, we pursued the research together for some time.

About that date Guthrie began to think that a Physical Society should be formed at which to read papers and discuss questions on Physics or Natural Philosophy, as it used to be called. There had for long been a Chemical Society, and there was also the ancient and dignified Royal Society. But a paper or communication had to be very novel to secure publication by the Royal Society, and there was little or no discussion on it.

Guthrie sent out a circular letter inviting opinions upon the subject. Some eminent philosophers doubted whether a fresh learned Society was required, but many younger men welcomed the idea, and a sufficient number were found willing to join.

Hence it became established, and a preliminary meeting was held in 1874. Dr. J. H. Gladstone, a well-known scientific man, was elected the first President. Our present Prime

Minister, Mr. Ramsay Macdonald, married Dr. Gladstone's daughter.

It so happened at that time that I had been making a new kind of voltaic battery, and had prepared a paper on the subject ; and Professor Guthrie paid me the great compliment of asking me to read this paper as the first communication to his new Physical Society. As the founder of the Society, he might well have reserved that distinction for himself, but he generously allowed it to me, a young and unknown man. A few words may be said on this particular research which had occupied me.

The Italian philosopher, Volta, discovered, in 1799, how to produce the wonder-working electric current. About 66 years before, in 1733, a brilliant young French scientist, Du Fay, made the great discovery that there are two kinds of electricity. Centuries before that time it had been found that particular substances, such as glass, wax, amber, etc., when rubbed had the power of attracting light objects, and were said to be electrified. It was the discovery of Du Fay that the electricity produced on wax by rubbing it with flannel was different from that produced on glass by rubbing it with silk ; also these two electricities when brought together in equal quantities destroyed or annihilated each other.

The progress of chemical science in later days, that is, about the beginning of the nineteenth century, had shown that material substance we call Matter cannot either be created or destroyed by us. When a piece of paper is burnt in the fire it disappears as paper, but the materials of which it is made combine with a gas called Oxygen, a component of the air, and form invisible gases called products of combustion. If, however, we gather all these together, with any ashes left behind, and weigh them, we find that the weight is exactly equal to that of the original paper and the oxygen used up in burning it.

Besides the substances we can touch, see, or weigh which we call Matter, there is something else which is also indestructible and uncreatable by us, which we call Energy. It is proved by everyday experience that when a material thing is in motion, it possesses a power of making useful or destructive changes in other things, which it does not do when at rest. Thus a bullet laid on a board makes no change in the board, but if fired from a gun and thus set in rapid motion it can make a hole in the board and tear wood out of it. In the same way, a motor-car in motion can effect useful changes by transporting materials or persons, or destructive effects by collision with other things.

The moving substance is said to possess energy of motion, or *kinetic energy*, from the Greek word for motion.

There is, however, another mode of manifestation of energy in which it is called potential energy, or energy of displacement. If we carry a heavy mass of any material to a height, that is, lift it up and place it (say) on a shelf, it has there no kinetic energy, or energy of motion ; but a little push may dislodge it, and cause it to fall and possess motion, or kinetic energy. Hence, when lifted up, although it does not then actually move, it possesses the possibility of motion, and is said to have *potential energy*.

But not only can we set in motion large masses of Matter, we can also set in vibration the atoms of which matter is composed. We call this atomic energy *heat*. Again, we can cause Electricity to move, and we call that an *electric current*. Hence, not only can we have Energy in the form of kinetic energy of large masses and potential energy as well, but we can have electric energy and heat or atomic energy. In short, just as Matter exists in many forms or many kinds of substance, so also we can have Energy in various forms as above described, and one form can be changed into another kind by suitable means.

The great discovery was, however, made in

the nineteenth century that we cannot produce Energy in one form without putting out of existence or destroying an equivalent amount in some other form. We can change it from one form to another according to a certain rate of exchange, just as we can change pounds sterling into francs or dollars.

Hence, Energy like Matter cannot be created or destroyed by us but only transformed, but we must also add that Matter of certain kinds can be changed into Energy of a certain kind. In short, Energy and Matter are merely different modes or aspects of a single entity.

To return, then, to Volta's discoveries at the beginning of the last century. Volta discovered that if a piece of zinc was made to touch a piece of copper without any rubbing, the mere contact of these two different metals electrifies them differently. The zinc becomes electrified positively and the copper negatively, and the same for pairs of other metals. He arranged all the common metals in a certain series, in which each was positive with respect to all below it. This is called Volta's series.

The difference of potential, as it is called, between zinc and copper in contact is about half to three-quarters of a volt. If a number of pieces of metal of any kind are arranged in a series, all touching each other, the difference of

potential of the two end-pieces is exactly the same as if these two were in contact alone without any others between them.

Volta then made another very important discovery. He found that if he placed one on the other a series of discs of zinc, copper, and wet cloth in the order zinc, copper, wet cloth, zinc copper, wet cloth, etc., a very large number of such triplets being piled on each other, he could then accumulate electric pressure or potential as much as he pleased. The two end-plates of the pile of triplets became oppositely charged. If these end-plates are joined by a wire, that wire becomes hot and exhibits magnetic powers, and is said to be traversed by an electric current.

An electric current consists of electricity in motion, and is therefore a manifestation of energy. Hence to create this energy some other form of energy must be put out of existence. The question then at once arises, What is the source of the electromotive force and of the energy of the electric current in a Volta pile?

This question excited a scientific controversy which lasted for nearly a century. Some thought that the exclusive source of the Volta electromotive force (E.M.F.) in the pile was the contact of the different metals, and others that it was in the chemical action, because it was soon observed that when the ends of the pile are

joined by a wire and an electric current is produced, the zinc plate begins to dissolve away. If it is pure zinc, it will not dissolve in dilute acid when the Volta pile is not producing a current.

The paper read by me on March 21, 1874, at the inaugural meeting of the Physical Society of London, founded by Professor Guthrie, was a small contribution to the controversy on the theory of Volta's pile. I wished to show that it was possible to make a Volta pile giving an electric current without any contacts of different metals or liquids. For this purpose I arranged in a row a number of pairs of glass cups. In one cup of each pair I placed dilute nitric acid, and in the other cup a solution of persulphide of sodium, and I bent over a strip of lead so as to dip into both cups of the pair. Placing the same solutions in the other pairs of cups, I bent over strips of copper to dip into the liquid in alternate cups, so that each pair of cups was connected to its neighbouring pair on both sides by these copper hairpin-shaped strips of copper, and each pair of cups was connected by a similar strip of lead. In this way I made a Volta pile of sixty cups, but the end-plates in each case were copper, and there was no contact or touching of different metals at any point. I therefore made a voltaic pile without dissimilar metals

or liquids in contact, and yet it gave an electric current when the two end copper-plates were connected by a copper wire, and it exhibited all the electric properties and powers of an original Volta pile. The paper gave rise to an interesting discussion, and I think it stimulated additional research on the subject.

It should have been mentioned that during the year 1874 I had been appointed by Dr. Frankland to take some share in the teaching work of the Advanced Chemical Laboratories, and for a short time I acted as his lecture assistant as well. This latter position was held by Mr. S. Newth, but on one occasion Mr. Newth met with a serious accident in preparing a lecture experiment, being badly burnt by some phosphorus, which is an extremely inflammable substance. To meet the difficulty of Mr. Newth's absence until he recovered, I took his place. The teaching work I did in the Advanced Chemical Laboratory gave me some very good experience, and as the following kind testimonial from my students will show, my teaching met with appreciation from them, and was marked by a very kind gift of books as well, which I remember were the three volumes of H. de la Rive's large treatise on Electricity, and two volumes of a German book on the same subject.

Copy of a letter received from twenty-nine of the Students in the Advanced Laboratory of the Royal School of Mines at the close of the Session 1873-74.

DEAR SIR,—Before separating for the Mid-summer Vacation, we, the undersigned Students of the Advanced Laboratory, desire to express to you our appreciation of your kindness and courtesy for the past six months during which we have been working under your supervision.

We have invariably found you willing to afford us information when appealed to ; and we sincerely regret that next year's course of study will prevent many of us from again working in the Laboratory with you.

In conclusion, we beg your acceptance of the accompanying books as a slight token of our esteem.—We are, Dear Sir, Yours very sincerely,

(Signed by twenty-nine Students
of the Advanced Laboratory).

To J. A. Fleming, Esq., B.Sc.

At the end of that session, in June 1874, it became necessary for me once more to take up some remunerative teaching position, in which I could support myself and relieve my parents ; and I accordingly became a candidate for the vacant position of Science Master at Dulwich College. Both Dr. Frankland and Professor Guthrie gave me very kind and most valuable testimonials, as reproduced below :

*Copy of a letter from Dr. Frankland, D.C.L., Ph.D.,
F.R.S., Professor of Chemistry in the Royal
College of Chemistry and School of Mines.*

ROYAL COLLEGE OF CHEMISTRY,
SOUTH KENSINGTON,

June 2, 1874.

MY DEAR MR. FLEMING,—Understanding that you are a Candidate for the appointment of Science Master at Dulwich College, I have much pleasure in offering my testimony to the especial qualifications which you possess for such a post. The breadth of culture necessary for the attainment of your Science degree at the London University, and the special training which you have received since in these Laboratories in Chemical and Physical Science, constitute an aggregate of qualifications rarely found in the same individual. In addition, however, you have also acquired considerable experience in teaching, first at Rossall College and afterwards in my Laboratory. The instruction which you have given during the past session to my advanced students convinces me that you possess in a high degree the faculty of communicating your knowledge to others in a clear and intelligible manner; and I cannot doubt that, should you be elected to the post you are seeking to attain, you will perform its duties with satisfaction to every one concerned.—Believe me, Yours sincerely,

(Signed) E. FRANKLAND.

Ambrose Fleming, Esq.

Copy of a letter from Professor Guthrie, Ph.D., B.A., F.R.S., Professor of Physics in the Royal College of Chemistry and School of Mines.

May 5, 1874.

MY DEAR SIR,—Hearing that you are a Candidate for the post of Physicist and Chemist at the Dulwich College, I have much pleasure in expressing my strong sense of your fitness for that office. Of your qualifications as a Chemist I need say nothing, as your position as Demonstrator in the College of Chemistry is ample guarantee of your chemical standing. Your recent paper to the Physical Society on the "Contact Theory of the Galvanic Battery" is an important contribution to science. My intercourse with you while we were recently prosecuting a joint research convinced me that your knowledge of Physics is both varied and accurate. Your Degree speaks for your general scientific culture.

I believe you to be in every way thoroughly qualified for the duties of the office you seek—Believe me, Faithfully yours,

(Signed) FRED. GUTHRIE.

Ambrose Fleming, Esq.

If I had followed up this candidature there is not any doubt that these influential testimonials would have stood me in good stead. But it had been brought to my notice that there

was at that time a similar vacancy at Cheltenham College, in the Military and Civil Department, for a Science teacher, and as this appointment was in some respects better than that at Dulwich, I withdrew my candidature for Dulwich, and entered for Cheltenham ; and I was successful in being appointed to this last-named College.

Cheltenham was at that period a very pleasant residential town, its chief street being a splendid boulevard or promenade, planted with trees on both sides. It had springs of medicinal waters, and, owing to the educational facilities of the town, had a large residential population of educated people, of good social rank, residing there for the sake of their sons and daughters. The large Boys' College or School was divided into two parts. One was an ordinary classical school, conducted on the lines of other British public schools. The Principal of that side was the Rev. H. Kynaston, M.A. The other part of the school, to which I was attached, was called the Military and Civil Department, and was chiefly occupied with training boys for entrance to the Army by success in the Woolwich examinations, and also for posts in the Civil Service, and those positions in the British Civil Service gained by competitive examinations. The Principal of this side was the Rev. T. A. Southwood, M.A.

I found that the Science teaching in this department needed improvement. I spent a large amount of time at first in making or preparing more chemical and physical apparatus, so as to demonstrate practically to my students the effects of things I told them. Those who have read Charles Dickens's novel, *Nickleby*, will remember that Mr. Squeers, the proprietor of Dotheboys Hall, a Yorkshire school, insisted on the necessity for combining very closely theory and practice when explaining to his assistant the method of his school. "We go upon the practical mode of teaching, *Nickleby*," said Mr. Squeers, calling up a boy, to whom he said, "How do you spell window?" "W—i—n—d—e—r," said the boy. "Quite right, now go and clean it," said Mr. Squeers. "That's our system, *Nickleby*. When a boy has spelt 'winder' he goes and cleans it." The only effective system of Science teaching is to allow the boys to see or do the things about which they are learning and, in fact, to follow Mr. Squeers' system.

The accommodation for Science teaching at Cheltenham College in those days was not very satisfactory. There was no properly equipped chemical laboratory, but a few boys could be taught at the back of the lecture-room, and I had a small room I could use as a

private laboratory. There was, however, a Museum with zoological and botanical specimens adjoining the classroom.

My chief work was class teaching, each lesson lasting for an hour, and sometimes four or five classes a day. In addition I did a small amount of private "coaching." Meanwhile, I pursued my own private studies and self-education. The large number of assistant and house masters on both sides of the College formed a Society in itself, and as there was much entertaining there was a great inducement to give time to social life rather than strenuous scientific work.

There is also in the town a large and well-known Ladies' College, at which two of my feminine cousins were students. The first Principal of this latter College was Miss Dorothea Beale, a woman of great organizing and administrative ability, and by her and by another lady, Miss Buss, who had a similar school in London, the education of girls was raised to a much higher level than before. They were both women of high and serious aims.

Needless to say, the two Colleges for boys and girls were "out of bounds" to each other, and their isolation as far as possible made complete. There was, however, a certain small amount of contact between them, as brothers and sisters or cousins were being educated simultaneously at

the two Colleges, and some parents resided at Cheltenham for a time to achieve this result.

No one ever knew who was the young lady student who had courage to write on a black-board of a classroom in the Ladies' College, in the absence of the teacher, an epigram as follows :

“ Miss Buss and Miss Beale
Cupid's darts do not feel.
How different from us—
Miss Beale and Miss Buss ! ”

Probably as a consequence she had an interview with the Principal, which left her a wiser though, may be, a sadder girl.

In my leisure time at Cheltenham when I had put my laboratory in order, I began to do a certain small amount of research work. I had possessed myself of a second-hand copy of the three volumes of Faraday's *Researches on Electricity*, and began to make myself familiar with the work of that prince of experimentalists.

The greatest of his discoveries, which Tyndall calls the Mont Blanc of his achievements, was the discovery of induced electric currents and of magneto-electric induction. By this is meant the discovery that when an electric current begins or stops in a wire, it will produce another brief or transitory electric current, called an induced current, in an adjacent wire.

Faraday also discovered that if a copper wire

circuit is moved near a magnet in such a way that the number of lines of magnetic force proceeding from the magnet which are linked with that circuit are changed, then an electric current is created in the wire. Just as rays of light are sent out in straight lines in all directions from a lamp, so also from a magnet there is sent in all directions in curved lines a radiation which Faraday called "lines of magnetic force." When a wire is so moved near a magnet as to cut across these lines, an electric current is created in the wire.

Faraday knew that our earth itself is a great magnet, and that rivers or streams of water are conductors of electricity. Hence he thought that the flow of rivers across the earth's magnetic field might produce electric currents in them. He tried an experiment in the river Thames by putting two metal plates in the water at opposite sides of the river, but was not able to detect a decisive result.

This, however, inspired me to try to find the predicted effect when using a much stronger magnetic force than that due to the earth alone. I possessed in my laboratory a large electro-magnet which I had made when at Rossall. I placed between the poles of this magnet a glass tube, down which I allowed to flow water acidulated with sulphuric acid, which made it

a good conductor of electricity. On the two opposite sides of this tube I had fixed strips of platinum foil attached to wires sealed through the glass.

When the acidulated water was allowed to flow down the tube, and the electro-magnet excited so as to produce a strong magnetic force across the tube, I found that these platinum plates became "polarized"—that means put into a condition in which, when joined by a wire, they could produce an electric current. In short, they were changed in some way, so that they acted like the zinc and copper plates in a voltaic cell or battery. This proved that a feeble current of electricity must have passed across from one plate to the other.

When at Rossall I had been taken by a relative on a visit to Edinburgh, where the meeting of the British Association was held in 1871, and had the first opportunity of seeing some eminent scientific men whose names were familiar to me, and whose personal friendship I subsequently came to enjoy, such as Sir William Thomson (afterwards Lord Kelvin), Sir James Dewar, and many others. This visit gave me my first introduction to scientific society, as well as the opportunity of seeing the beauties of the city of Edinburgh and some of those of the surrounding country as well.

Then, again, when at Cheltenham I attended a meeting of the same Association, held in Bristol in 1875, and read my first paper to it, describing the above-mentioned experiments proving the production of induced electric currents in moving liquids. It was clear then that Faraday's assumption was correct, and that the flow of ocean currents or of rivers across the earth's lines of magnetic force must create electric currents in the water.

During the time I was at Rossall and at Cheltenham I had several opportunities for foreign travel. One of the qualities I inherited from my mother was an intense love of natural beauty in scenery and also interest in travel. When I was a schoolboy my parents had taken me to Switzerland for the first time. We crossed the English Channel to Havre, and went through Rouen to Paris, and thence to Geneva and Chamounix, where for the first time I saw with delight the snow-covered Alps, with Mont Blanc as their white-robed queen. This must have been in 1867 or 1868, for I remember that in Paris we saw the Emperor Napoleon III. drive up the Champs Elysées, and we visited the tomb of Napoleon Bonaparte and the Louvre and the Palace at Versailles. The novelty and beauty of the Swiss journey was to me a surpassing delight. Then when at Rossall,

after the defeat of the French in the Franco-Prussian War of 1870, I went again with a cousin for a walking tour to Switzerland. The battle of Sedan had then been fought, the French army surrendered, the Emperor Napoleon was a prisoner of war. Paris had been besieged, and the King of Prussia had been crowned German Emperor at Versailles. After that a Communist rising took place in Paris. I remember seeing the fragments of the Column Vendôme lying on the ground, and the evidences of battle in the streets of Paris.

Also when at Cheltenham I took a long tour in the Austrian Tyrol with Mr. and Mrs. Cumming. Mr. Cumming was one of the mathematical masters on the classical side of Cheltenham College, and the author of a short treatise on the mathematical theory of Electricity. In that journey we visited Innsbruck and the salt mines of Salzburg, and climbed the steep slopes of the Gros Venediger mountain. On the way out we had passed through Nuremberg and Munich, visiting in Nuremberg the prison of the Inquisition with its shocking instruments of torture, a reminder of mediæval cruelty, especially the famous Iron Virgin, and in Munich we saw the celebrated picture galleries.

Meanwhile, the question of my future work had begun to press for attention. The social

life at Cheltenham was very agreeable, but it opened up no prospect for better things. An assistant master at a public school may aspire to become a house-master, but this simply means becoming a sort of hotelkeeper with fifty boys as boarders, and I had no taste for such a career. Headmasterships are restricted mostly to classical scholars who are also in clerical orders.

During my time at Cheltenham College I had been attempting to follow the work of one of the greatest of the nineteenth-century physicists, namely, Professor James Clerk Maxwell. He had published, in 1873, his great treatise on *Electricity and Magnetism*, in which he had applied his brilliant mathematical powers to translate Faraday's electrical ideas into mathematical language ; and the attempt to read Maxwell's books and papers had brought home to me my own defective mathematical training.

I therefore desired to go to Cambridge and work under Maxwell, who had not long before been appointed there as the Cavendish Professor of Physics. My parents and friends thought me very foolish to surrender my good appointment at Cheltenham, where I had an income of £400 per annum, but I knew well it was a blind-alley occupation, and that it was useless to stay on. I aspired to better things than an assistant mastership in a public school.

So in the summer of 1877 I sent in my resignation, which was accepted with regret, as the following testimonials from the two Principals will show :

Copy of a letter from the Rev. H. Kynaston, M.A., late Fellow of St. John's College, Cambridge, and Principal of Cheltenham College.

July 13, 1877.

MY DEAR FLEMING,—I have been expecting to see you on my way to or from College every day since I had your letter, that I might tell you how very sorry I was to hear that you proposed to leave us. But I quite understand and appreciate your motives. When I heard of your engagement in London as examiner of the same Board with the greatest physicists of the present day, I felt sure that we could not retain your services here much longer. I can only hope that you will not feel that you have wasted time here, or if you should, that you will set against that loss the good which you have done to your pupils.—Believe me to be,
Yours sincerely,

(Signed) H. KYNASTON, *Principal*.

Copy of a letter from the Rev. T. A. Southwood, M.A., Principal of the Military and Civil Department of Cheltenham College.

July 12, 1877.

MY DEAR FLEMING,—I have no doubt but that your decision to graduate at the

University of Cambridge is a right decision. I feel sure that your attainments, talent, and power of work will stand you in good stead, and that you will achieve all you desire as regards University distinction and rewards. During the three years of work here you have shown great interest in the boys who have come under your teaching, and what is not always the case, you have preserved admirable discipline, and cases have been rare when you have found it necessary to seek my aid. Your resignation places me in some difficulty, the difficulty being the finding of a competent successor. Men of the highest attainments in scientific knowledge are plentiful enough, but in combination with teaching power and tact in the management of pupils are scarce. If you can aid me in securing the right man I shall be obliged to you.—With every good wish for your future success, Believe me,
Yours very sincerely,

(Signed) T. A. SOUTHWOOD.

As neither my father nor any relatives could assist me financially, I had first to consider ways and means before I could surrender my position at Cheltenham College. First, I determined to endeavour to gain some scholarship or exhibition at Cambridge, and I therefore entered myself as a candidate for an open entrance science Exhibition or Scholarship at St. John's College of £50 for three years. I went up to

Cambridge at Easter 1877, and sat for this Exhibition.

My training in Chemistry and Physics at the Royal College of Chemistry stood me in good stead, and I was successful in winning this Scholarship against all competitors. I had also saved about £400, and my good friend Professor Guthrie promised me the assistance of an Examinership to the Science and Art Department which would secure another £50 per annum. Thus provided, I burnt my boats and severed my connection with Cheltenham College as Science Master in July 1877. But entrance at Cambridge meant passing an examination called the Little-go, which involved papers on Greek, Latin, and Theology as well as Elementary Mathematics.

As all my small classical knowledge had become very rusty, I had to get it up again, and I therefore located myself in August at Eastbourne in a boarding-house, to work it up. I had to prepare a Greek play by Euripides and an oration by Cicero, a Greek gospel (St. Matthew), and Paley's *Evidences of Christianity*. But this, by a little hard work and some "coaching," was accomplished, and in October 1877 I went up to Cambridge as an Exhibitioner of St. John's College, having provided myself with the means of support for the three and a half years of Cambridge study necessary for a degree.

CHAPTER III

CAMBRIDGE UNIVERSITY LIFE

ST. JOHN'S COLLEGE, Cambridge, of which I thus became a member in October 1877, was founded by Lady Margaret, the mother of our Henry VII. and grandmother of Henry VIII., whose numerous experiments in matrimony have made him notorious. It was dedicated to St. John the Evangelist, the eagle-eyed apostle, and hence its crest is an eagle, and its coat of arms contains the portcullis and rose of the Tudor family.

At this date it comprises four courts entered from the street through a stately gateway of red brick, ornamented with Tudor Arms. The chapel stands on one side of the first court. The original chapel became too small long ago, and was replaced in the last century by a larger and more beautiful building, designed by Sir Gilbert Scott.

Passing through a corridor which separates the kitchens from the dining-hall, we reach the second court, with its Fellows' Combination Room on the first floor on one side.

The chaste third court is smaller, and adjoins the river Cam. Over this is a bridge modelled on the Bridge of Sighs at Venice, which leads to the much later New Court, and a cloister on one side of this opens on the gardens. Around these courts are the rooms of undergraduates and dons or fellows. Each set comprises a sitting-room, bedroom, and gyp-room; in the case of fellows or tutors more than one sitting-room. Undergraduates at my time at a Cambridge College could either reside in rooms in College, or else in registered lodgings in the town.

For those who wished to pay most attention to the social life of the College, it was better to reside in College; but if one wished, as I did, to give all possible time to serious study, it was an advantage to be in lodgings, as one was not so liable to be interrupted by callers.

Accordingly, I engaged rooms in a house in Portugal Place, quite near St. John's College. Residents are waited on in College by men-servants, called gyps, and some of the gyps rent houses in which members of the College can engage rooms. This is a great advantage, because one can send messages into College by the gyp, and if one is ill or desires dinner in rooms, the gyp can order it at the College kitchen.

Every College has a large dining-hall in which the undergraduates and dons dine—the dons

at a high table, raised on a platform a little above the floor-level. There are one or more dinners in the evening, at which all dine in academic gowns, and enjoy an excellent dinner at a very moderate charge.

On the stained-glass windows of the hall are the names of some memorable Johnians—such as Roger Ascham, the tutor of Lady Jane Grey and Queen Elizabeth ; also Wordsworth the poet, and Palmerston the Premier. Over the Master's chair on the dais hangs the portrait of the foundress, Lady Margaret ; and other portraits of more modern members decorate the walls. The master in my time was Dr. Bateson, one of whose sons became an eminent naturalist. Bateson was a large man with a large family, but he was succeeded at the Lodge by Dr. Taylor, who, I believe, was a bachelor, and he again by Sir Robert Scott.

There is also a St. John's College at Oxford, but it is dedicated to St. John the Baptist.

Many persons will remember the witty remark of an undergraduate to a freshman friend. The Master of St. John's, Oxford, at one time, was a small man but rode a big horse. A freshman seeing him ride down the High Street asked his friend who he was, and the other replied, " That is the Head of John the Baptist on a charger."

Every undergraduate has to be attached to a

College tutor, who gives him no instruction, but is *in loco parentis*, or in place of a parent. It is a purely nominal relation, but it is usual to call on one's College tutor at the beginning of each term and pay him a fee, and ask his permission if it is necessary to leave Cambridge in term-time. My College tutor was Dr. Parkinson, who in his day was a Senior Wrangler, and beat Mr. William Thomson, afterwards Lord Kelvin, who later became one of the greatest mathematicians of the nineteenth century.

It has frequently been the case that a Senior Wrangler has been less celebrated in after-life than some one second or lower on the list. Parkinson had a pawky kind of humour, and there was a story which illustrated it. The New Court of St. John's opens on to the gardens by a large arched entrance, on the top of which is a stone eagle, the crest of St. John's. One day there was a ladder left leaning against the arch, and an enterprising undergraduate climbed up it and clothed the eagle in a scarlet jacket or "blazer" worn by the members of the Lady Margaret (or St. John's) Boat Club. Just then the ladder fell down, and left the climber marooned on the arch forty or fifty feet above ground. Dr. Parkinson's rooms overlooked the spot, and he saw what had happened whilst dressing. As soon as he was dressed, he sent

the gardener to rescue the unfortunate man. Some time after, the climber came to say farewell as he was going down, and Parkinson asked him what he was intending to do. He replied he was going to be a missionary, and Parkinson said, "Well, sir, I hope you will have more success in other climes (climbs) than you have had here."

As soon as I had passed the Little-go examination, which I did in the first October term, I began to read mathematics with Mr. W. H. Besant, one of the famous mathematical "coaches" at Cambridge and a Fellow of St. John's.

The system of teaching at Cambridge is conducted by three classes of teachers. There are in the first place College lecturers, who give lectures on certain subjects on which they are proficient. Then there are the University professors and readers. These very eminent men are not attached to any particular College, but as their lectures are of an advanced nature, the general run of undergraduates do not attend them, and the professors have very small audiences.

The majority of the hardworking students, and those who desire to take a good place in the final Degree examinations, called the Tripos, and, if possible, to secure a Fellowship, gain their teaching from special experts called "coaches." There was also in my time a rule

by which students at one College could attend lectures by lecturers or readers at other Colleges. The chief mathematical "coaches" at the date I was at St. John's were Mr. W. H. Besant, with whom I read, also Mr. E. J. Routh and Mr. R. Webb. These gentlemen did not give lectures in the ordinary sense of the word. The student went to them at an appointed time, and the "coach" gave him an examination paper of questions and supplied a hint or two as to how the questions were to be solved, and also marked certain chapters or parts in a text-book to be read. The men were taken either alone or in couples. Then the student went back to his own room and tackled the paper of questions, and read as requested. The next day we took our results to the coach, who noted successful answers, and gave a further hint as to the solution of unsolved problems. The coaches had great experience in forecasting the kind of question likely to be put in Tripos exams., and it would have been quite impossible to obtain a high place without their aid.

In addition to the Tripos exams., which take place at the end of the three or three and a half years, there are College examinations which take place once or twice a year, and on the results of which scholarships and prizes are awarded.

One of the aims of every serious student is to

become a foundation scholar of his College, which means an award of £50 to £100 or so for one or more years. I was successful in my second year in becoming a scholar of St. John's. The scholars dined in Hall at a separate table called the scholars' table.

In addition, I won several prizes, such as the Hughes and Wright prizes, and was thus able to "pay my way" without difficulty. At the date of my undergraduate career (1877-81) a student who lived a quiet life and did not indulge in any expensive amusements could cover all his expenses for about £200 per annum, but at the present time this figure would no doubt be exceeded.

For the first six months of my undergraduate life I made all possible efforts to advance my mathematical acquirements to the point at which I could profit by the attendance on the lectures of Professor Clerk Maxwell, to work under whom it had been my chief object in coming to Cambridge.

For some years previously to about 1870 there had been a feeling that the study of Physical Science in Cambridge had not been sufficiently cultivated. The great advances made in various branches, and especially in the sciences of Electricity and Magnetism, needed better appliances and accommodation for their teaching.

Accordingly, the seventh Duke of Devonshire, at that time Chancellor of the University, had intimated his intention to bestow on the University such benefactions as would enable an adequate physical laboratory to be built and equipped and a Professor of Physics appointed. The University Authorities then wisely resolved to secure the Professor first, and let him design and arrange his own laboratory.

On March 8, 1871, their choice fell on James Clerk Maxwell as the first Cavendish Professor, and he consented to occupy the chair.

James Clerk Maxwell was descended from an old Scottish family, many of whose members were talented and remarkable. He was the only son of Mr. John Clerk Maxwell, a Scottish laird, and in his brilliant son the family genius came to its climax ; for he exhibited as a child traces of that originality and power which made him in his prime one of the greatest intellects that our nation has produced. James Clerk Maxwell was born in Edinburgh, at 14 India Street, on June 13, 1831, and after a school and college education at Edinburgh, he entered Cambridge in 1850, first at Peterhouse College, but soon migrating to Trinity. Even as a boy of fourteen years old, he had made remarkable investigations in mathematics which had been described to the Royal Society of Edinburgh.

Hence, when he came up to Cambridge, at nineteen years of age, he was not so much a student as a master in certain subjects. All his contemporaries recognized his astonishing abilities and vast information. Hardly any subject could be introduced for conversation to which he could not contribute new and interesting information.

At the final Mathematical Tripos examination he came out Second Wrangler ; the Senior being Dr. E. J. Routh, later a most eminent "coach," but they were bracketed together in the Smith's Prize examination. Shortly afterwards he became a fellow of Trinity. He then began those epoch-making mathematical investigations on electricity which gave him his world-wide fame.

In 1864 he sent to the Royal Society of London a paper, published in their *Transactions* for 1865, entitled "The Dynamical Theory of the Electro-Magnetic Field." After Maxwell's death, in 1879, I remember Sir W. D. Niven, himself an eminent mathematician, saying to me in all seriousness that he thought this paper of Maxwell's, by its wonderful originality, was one of the very greatest productions of the human mind. In this paper Maxwell first showed the possibility of existence of electromagnetic waves, long afterwards by Hertz experimentally produced, and now everywhere

used in world-wide wireless telegraphy and telephony. Maxwell had at that time begun to write his great treatise on *Electricity and Magnetism*, which was published in 1873, and attracted intense attention from the whole scientific world.

Meanwhile, the Cavendish Laboratory had been built and equipped under Maxwell's direction, and courses of lectures were begun and a few men enrolled as research students. Dr. William Garnett was appointed Demonstrator, and a man named Pullen as mechanic. It was to have the privilege of the personal teaching of this remarkable man, and to work under him in the Cavendish Laboratory, that I gave up my post at Cheltenham College, and no wiser step could I have possibly taken.

As a teacher Maxwell was difficult to follow. He had a paradoxical and allusive way of speaking, and he saw so much deeper into scientific problems than ordinary persons that his utterances often appeared obscure. The only method to pursue was to try and remember carefully what he said, and the meaning of it would probably become clearer after a time.

At the beginning of 1878 I visited the Cavendish Laboratory, and asked Maxwell's advice as to the practical work I should do in the laboratory.

Ten years or more before that time, when

the laying of submarine electric telegraphs had begun, and when the Government were about to take over into control the land telegraphs of Great Britain, the matter of making electrical measurements had become important. Just as we have standard yard measures and standard pounds and gallons preserved at the Standard Office, with which all commercial standards of measurement have to conform in order to avoid fraud, so in electric work we must have certain legally established standards for electric quantities, such as electric current, electric voltage, and electric resistance.

The unit of electric current is called 1 ampere, that of electric pressure 1 volt; and a wire is said to have a resistance of 1 ohm, if a pressure of 1 volt sends through it a current of 1 ampere. These units are called after the names of eminent electricians, such as Ampère the Frenchman, Volta the Italian, and Ohm the German.

The British Association Committee on Electric Units, of which Maxwell was a member, had constructed certain coils of wire of various materials, each of which was supposed to have a resistance of 1 ohm at a marked temperature. These coils had been deposited in the Cavendish Laboratory. In the course of time they had all altered slightly, and moreover there was reason to believe that they were all about $1\frac{1}{2}$ per cent.

too small. Accordingly, Maxwell suggested to me to compare them with one another and endeavour to find what was the most probable value of the electric unit of resistance called the B.A. unit.

If, for instance, we had a dozen yard measures all very slightly different yet each claiming to be a standard yard, we could by certain processes determine the most probable length of 1 yard from them.

Electricians already had an instrument for comparing electric resistance which was called a Wheatstone's Bridge. It had nothing to do with a bridge over a river, and it was not invented but only used by Sir Charles Wheatstone. I soon found, however, that it needed modification to be effective for comparing resistances by a method suggested by Professor Carey Foster, and I designed a suitable appliance. As it had the shape of a well-known musical instrument, Maxwell was accustomed to call it "Fleming's Banjo." He had it constructed in the engineering workshops, which were under Professor James Stuart, the Professor of Mechanism, and it remained for years a much-used instrument, in the Cavendish Laboratory. I described its construction in a paper in the *Philosophical Magazine* for February 1880.

At the same time I began to attend Maxwell's

lectures. He usually gave a course on Heat or Thermo-Dynamics in the October term, a course on Electricity in the Lent term, and one on Electro-Magnetism in the May term. The reason for the small attendance on Professor's lectures at Cambridge has already been explained. For some terms, in the years 1878 and 1879, Maxwell had as his only audience an American gentleman, Mr. Middleton and myself. Nevertheless, he gave us the same care and valuable teaching as if he had had an audience of one hundred students.

After one lecture, at which Maxwell had given us some especially original instruction, Mr. Middleton turned to me and said, "Sir, this man (meaning Maxwell) is great! He does not come here and tell us what he has read out of books. He gives it to us hot from his brain."

What was particularly striking about Maxwell's lectures was his extreme ingenuity of even the smallest experiment or construction of common apparatus. He had a vast knowledge of the history of Science and could relate some anecdote or incident about very familiar facts which invested them with a new interest. He never left us with the idea that a particular subject was exhausted of all novelty, but with the feeling that there was much more to be learned about it.

During the last years of his life he was engaged in editing the unpublished scientific papers of Henry Cavendish, an eminent electrician, whose work was done about the fourth quarter of the eighteenth century (A.D. 1771-81). Cavendish had a most unusual objection to publishing the results of his experiments, but he placed them on record in written manuscripts, and these remained in the possession of the Duke of Devonshire, who had requested Maxwell to examine and publish them. Maxwell repeated practically all the principal experiments described by Cavendish.

Cavendish had very few experimental appliances, and his only method of comparing the electrical resistance of substances or wires was by taking electric shocks through them, and adjusting them until the shocks appeared to be equal. Cavendish had a valet called Richard, who was made to add to his duties by acting as a shock-meter. I remember assisting Maxwell in a similar capacity one afternoon, to decide whether two electric shocks taken through different circuits were equal or different in intensity.

Everything which Maxwell did or said bore the mark of genius and the utmost care. On one or two occasions I remember he gave a university lecture to me as his only auditor. My other

usual colleague was absent. Sir William Crookes had been to Cambridge, and given a lecture one evening in Maxwell's lecture-room, and the next morning was packing up his apparatus, when Maxwell entered in his M.A. gown. After a few words with him, Crookes said, "Well, I must be going now, as I suppose your class will be coming in directly." I had already come in, and was seated in the front bench. Maxwell looked at me with a slightly ironical smile, and said, "I think the class is already here."

Nevertheless it was an inspiration and privilege to have enjoyed for two short years the teaching of such a mind as Maxwell's. Little did we think how soon we were to lose it !

Needless to say, I took the most careful notes of all his lectures, and in September 1931, at the centenary of his birth, held at Cambridge, I presented these bound volumes of notes to the Cavendish Laboratory.

Maxwell's mother died, when he was quite young, of that terrible disease cancer, and the same death-dealing ailment began to lay its cold hand on him in the early part of 1879. His strength and vitality began to fail. In May 1879 he delivered his last university lecture, and gave in the last two or three lectures a beautiful method of solving a certain class of electrical problem, which I afterwards explained

and extended in a paper to the Physical Society. He went as usual that summer to his Scottish estate, but in October the sad news came to us all in Cambridge that this great intellect was destined soon to pass away from us. He died at Cambridge, on November 5, 1879, to the unspeakable grief of all his friends, and of the whole scientific world. Strange to say, my father died five days later, on November 10, after a short illness, and was interred in Highgate Cemetery on November 14.

In the summer of 1879 I had taken the degree of Doctor of Science in the University of London, the subject being "Electricity treated Experimentally." At that date not many men had been awarded this degree.

The examination for Doctor of Science in the University of London for experimental subjects was partly by written answers to printed questions, and partly by experimental work in a laboratory. Also a thesis or essay had to be written on some subject not announced beforehand. In preparation for this, I wrote myself essays on about twenty different subjects, which I thought might be set, but none of them were actually given. The subject set was, however, one to which I had paid a good deal of attention, and I acquitted myself sufficiently well to be granted the degree ; and the result was an-

nounced some time in July 1879, as I remember it was a few months before my father died, and he was much gratified with my success.

As the death of my father left me with his affairs to settle as the eldest son, it interfered for some time with my mathematical studies, and I therefore resolved to take my Cambridge degree by sitting for the Natural Science Tripos, in which I felt fairly sure of a good place rather than in the Mathematical Tripos. This came off in the summer of 1880, and I was fortunate to obtain a First Class in two subjects, Chemistry and Physics, and was recommended for the B.A. degree. In preparation for this examination I attended the lectures of Sir George Stokes on Physical Optics. He was Lucasian Professor of Mathematics, the chair of Sir Isaac Newton.

After Maxwell's lamented death, the question of his successor became urgent. Who could possibly be able to succeed him? Before long it was ascertained that the third Lord Rayleigh, who was in his day a Senior Wrangler, and one of the most eminent of living physicists, would be willing for a time to take the chair. On his accession he at once began to direct his attention to the electrical units. I had by that time completed my work on the British Association Standards, and embodied the results in a chart which showed for each coil at what temperature

it most nearly represented the true B.A. unit. Lord Rayleigh, assisted by Sir Arthur Schuster, and later by Sir Richard Glazebrook, began their important work to decide how far this mean British Association unit, as fixed by my chart, had the theoretical or absolute value it should have had. It was found to be about one and a half per cent. too small, and as the result of much work at Cambridge and elsewhere a final more exact standard ohm was constructed and legalized for general use.

In December 1880 Professor James Stuart, Professor of Mechanism, offered me the position of demonstrator or reader in Mechanism under him, and to assist in the teaching in the drawing office of his Engineering Laboratory. This position I was glad to accept, as it enabled me to continue to reside in Cambridge. One of the clubs or societies to which I belonged was the Cambridge Science Club, commonly but irreverently called the "Stinks Club" by classical men. It comprised the senior Science students at various Colleges. We met on Saturday evenings at one another's rooms, and the host had to provide tea, coffee, and sardines on toast, and to read a paper on some scientific subject, which was afterwards generally discussed.

In 1876 Alexander Graham Bell, whose

father had been Lecturer on Elocution at University College, London, invented and patented the speaking telephone. In 1878 David Hughes had invented the microphone, more particularly described in a later chapter. In one form it consisted of two rods of coke laid lightly across each other. This loose contact was joined in series with a battery of one or two cells and a Bell telephone. The smallest shake or vibration given to the loose joint caused a sound in the telephone. Hughes called this a microphone, because he thought it enabled small sounds to be magnified.

One experiment which was popular at that time was to catch a common house-fly, and shut it up in an empty match-box and put a microphone on the box. It was then said that one could hear the fly walking about in the match-box with a sound like the tramp of an elephant. However, it was found that the same sounds were often heard when the fly had been dead for some time, or had escaped from the box, and they were therefore not due to the fly's peregrinations but to slight vibrations of the microphone by noises or shakes in the room.

At that time the invention of the telephone and microphone excited great interest, and on one occasion I read a paper to the Science Club on it. During part of the year 1881 I assisted

Professor Stuart in organizing the mechanical workshop and engineering department.

About that time the City of Nottingham had built a large College called University College, intended to provide popular and higher education for the people. Professor James Stuart had originated a scheme for popular education called the University Extension system, in which University graduates gave courses of lectures on various educational subjects in different towns, and so brought the advantages of higher education at small cost to young men and women who would not otherwise have been able to obtain them. This College at Nottingham was intended to render more permanent these advantages. It was intended that there should be popular evening lectures on scientific and literary subjects, and also more systematic day courses for middle-class students who could give time to them.

Professor Stuart had very great influence with the organizing committee, and it was suggested I should stand as a candidate for the Professorship of Physics and Mathematics, which I accordingly did. Largely, I believe, through Professor Stuart's influence and the testimonials I had previously received to my experience as a lecturer and teacher, I was appointed to that position in the summer of 1881.

Before concluding this chapter on my Cambridge life I may add that in 1882 I had the very great pleasure of being elected a Fellow of St. John's College. I sat for an examination some time in the summer of that year. On receiving with joy the telegram announcing my success, I hastened down to Cambridge and went through the ceremony of admission by the master, Dr. Taylor, in the Combination Room, and dined afterwards with the master and fellows in Hall. The prize fellowships continue for six years as regards emoluments.

Later on I took the M.A. degree. Forty-five years after, in February 1927, I had the still greater honour of being elected an Honorary Fellow of St. John's College. According to the statutes of the College, the Council may, at a meeting to be held for the purpose, and by a resolution in which not less than nine votes shall have concurred, elect to an Honorary Fellowship any person distinguished for learning or public service.

In 1912 I had the privilege of advising my College on a scheme of electric lighting of the College, when a public electric supply had been established. My scheme was carried out, and I believe it has proved perfectly satisfactory up to the present day.

CHAPTER IV

THE EARLY DAYS OF THE TELEPHONE AND ELECTRIC LIGHTING

IN July 1881 I went to Nottingham, to take up my duties as the first Professor of Mathematics and Physics in the University College of that city, built and equipped by the Town Council. It was a handsome building, in the centre of the town.

In 1928 a still finer building was erected on a different site, presented by Sir Jesse Boot, and it was opened in that year by H.M. the King. It forms now a University College in every respect worthy of a great city. Nottingham has always been famous for the public spirit of its chief inhabitants.

My first duty was to attend to the provision of experimental apparatus, as the Council had wisely left that to be arranged by the Professors. I had as an assistant a Nottingham man, Mr. Simpson, who was skilled in matters connected with lecture illustrations, and my first care was to provide the necessary apparatus for public lectures in the large theatre.

This original College was officially opened in the autumn of 1881 by the late Prince Leopold, the youngest son of Queen Victoria, and work began in it in September of that year. I had as one of my colleagues Dr. F. Clowes, the Professor of Chemistry, who took his degree of B.Sc. in London the same year that I did, and later on became consulting chemist to the London County Council.

One of the first public lectures was on Solar Eclipses, as I have always found that Astronomy is a subject which is popular with general audiences. Mr. Simpson and I arranged two oxy-hydrogen lanterns, by which we were able to depict on the screen the whole striking appearance of a total solar eclipse, one of the most impressive phenomena in nature.

Although I was able to start to some extent the University teaching at Nottingham, I did not remain there more than a few months, but had in the early part of 1882 an invitation to occupy a position in London, and one which I was only too glad to accept. I was succeeded in the Chair by Dr. W. Garnett.

During the time I was at St. John's, as already mentioned, the great inventions of the telephone and microphone were made.

Graham Bell had solved, in 1876, the problem of transmitting human speech along a telegraph

wire. His telephone was, like many other great inventions, remarkably simple. It consisted of a steel bar-magnet, on the end or pole of which was a little short rod of iron wound over with a coil of fine silk-covered copper wire. Nearly touching this iron core was a flat circular thin steel plate, about 2 inches in diameter, called a diaphragm. The plate was fixed at its edges to a wood or ebonite case which contained also a magnet. This telephone acted both as a transmitter and receiver, if two such telephones had their respective copper coils connected by a pair of wires. If a speaker shouted loudly to the diaphragm of one telephone it was set in vibration, and this created electric currents in the telephone coil which were transmitted along the line wires to the coil of the other telephone, and this in return caused vibrations of the diaphragm of the receiving telephone similar to those of the transmitter, and so reproduced the speech sounds at a distance.

Lord Kelvin, then Sir William Thomson, was in the United States of America in 1876, and at an International Exhibition in Philadelphia he saw and heard Bell's telephone for the first time, and brought back with him to England a sample.

The British Association was held at Glasgow in that year in August, and I was present in the

Section A. when Sir W. Thomson described with enthusiasm Bell's invention. He said he had heard the words "To be or not to be" transmitted with the utmost distinctness, and also the sentence "The Americans in London have made arrangements to celebrate the Fourth of July" sent along a telegraph wire.

The Bell telephone was a good receiver, but a poor transmitter; and whilst no one has yet been able to improve upon the original Bell telephone as a receiver, many inventors at once attacked the problem of inventing a better transmitter. Some time previously Edison had discovered that if a little pad or button of lampblack or soot, which is essentially carbon, was placed between two metal discs, its electrical resistance could be changed by slightly squeezing the plates together. It then occurred to him that, if one of these plates was a thin metal disc, and the other a thick rigid plate, then when speech was made to the thin plate the changes of air pressure would more or less compress the carbon button, and similarly affect the intensity or strength of an electric current sent through it from a battery. Hence if this button and battery were connected by two leading wires to a distant Bell receiver, speech could be transmitted.

Edison soon found that his ideas were correct,

and he thus made what was called the carbon button transmitter. Nevertheless, he was not independent of Bell's invention for a receiver, and he proceeded therefore to invent a receiver which should be outside any claims in Bell's patent.

Edison obtained a British patent for his invention of the carbon button transmitter in 1877. In 1878, as has been already mentioned, David E. Hughes, who was an inventive genius of the highest order, discovered the properties of a loose contact between two pieces of coke or hard carbon, and this was soon developed into a microphone or speech transmitter. He did not apply for any patent, but it led to an acute controversy with Edison, who accused Hughes of pirating his carbon button invention. They are, however, not identical.

At or shortly after that time, in 1879, two telephone companies had been formed in London to operate the telephonic inventions respectively of Bell and Edison, and certain questions were in dispute between them. The chairman of the Edison Telephone Company of London was the Rt. Hon. E. P. Bouverie, and the Secretary, Mr Arnold White, a cousin of the author of these *Memories*. Mr. Edison's representative in England was Mr. E. H. Johnson. As litigation was pending with the London Company formed to work the patents of

Graham Bell, it was considered necessary for Mr. Johnson to return to the United States to collect evidence, and meanwhile I was appointed to act as a scientific adviser to the Edison Telephone Company, in November 1879. The London Offices were at No. 11 Queen Victoria Street.

Graham Bell had clearly foreseen that the great use of the telephone would be to form telephone exchanges, by which a number of persons called "subscribers" could intercommunicate with one another through an establishment called a telephone exchange, to which all the wires or circuits of subscribers were connected.

To continue our story, we must now go back a good many years in scientific history to explain the development of telegraphy. Although the idea of an electric telegraph to transmit messages had been present in many minds for nearly a century, it was first reduced to actual practice in England by Cooke and Wheatstone, and in the United States of America by Morse.

As various methods of signalling were invented, and telegraphy perfected, joint stock companies were formed to work the inventions, and telegraph wires laid down to intercommunicate between towns and along railways. The electric telegraph began its practical existence about the time when Queen Victoria came to the throne, and soon proved its use to the public.

Then, in the 'sixties of the last century, an agitation began against the growth of "monopolies," by which was meant that services of public utility should not be conducted for private profit but for the benefit of the nation as a whole. The result was that, in 1868 and 1869, the Government of that day passed Acts of Parliament authorizing them to buy up the plant and goodwill of the various telegraph companies for about £10,000,000 sterling, and put the whole business of electric telegraphy in the hands of the General Post Office to conduct and extend it.

In order to protect themselves from possible rivalry, these Acts were so skilfully worded that they rendered it illegal to conduct any mode of intercommunication by electricity for public service for consideration or payment.

When the telephone was first invented, one of the technical officials of the General Post Office had declared it to be a "toy," and of no use, but when the first attempt was made to establish a telephone exchange, the General Post Office took alarm lest the new mode of communication should interfere with their telegraphic receipts. Hence, in 1879 they flung a bombshell into the telephonic camp by declaring that a telephonic line was a telegraphic line within the meaning of the Acts of 1868 and 1869.

Accordingly, they gave notice to the telephone

companies of this decision, and an important legal action was tried in 1880, under title of "The Attorney-General *v.* The Edison Telephone Company of London, Limited." The case was heard by Mr. Justice Stephen and Baron Pollock. Scientific witnesses were retained on both sides. Those for the Crown were Mr. (afterwards Sir) W. H. Preece, Sir Charles Bright, Messrs. W. H. Barlow, Latimer Clark, D. E. Hughes, and Warren de la Rue. The Telephone Company retained Lord Rayleigh, Sir W. Thomson (afterwards Lord Kelvin), Sir G. C. Stokes, Professor Tyndall, Dr. J. Hopkinson, and Dr. J. A. Fleming. Of all these, the present writer is the only survivor at this date, 1934.

In spite of the arguments urged by counsel, that what the Government bought was merely the telegraph industry and methods as they stood in 1869, and not every possible means of intercommunication by electricity which the wit of man might ever invent, and in spite of the evidence that telephony was not even thought of at the date named, the Judges gave decision in favour of the Crown. The case, unfortunately, never went to the Court of Appeal nor to the House of Lords.

The General Post Office offered the telephone companies a licence for thirty years in consideration of payment of 10 per cent. of their profits.

The telephone companies did not care to risk more capital in contest with the Crown, and accepted the terms offered. The Bell and Edison Companies put together all their patents and united into one company, called the United Telephone Company Limited, and later the National Telephone Company. The Government undertook to lay and maintain the trunk lines for interconnecting the various exchanges.

Thus, in consequence of the Acts of 1868 and 1869, the control of this new and wonderful industry of telephony passed into the hands of the General Post Office, and its development was thereby retarded in Great Britain, in contrast with that in the United States, where it was not under Government control. The fuller statement of the consequences of this legal decision are set out in the Author's book, *Fifty Years of Electricity* (Iliffe & Co., London).

Meanwhile, another great electrical invention had been coming to fruition. At the beginning of the nineteenth century Sir Humphrey Davy at the Royal Institution had first discovered how to produce a brilliant light by means of an electric current passed between the ends of two rods of coke brought into contact, and named it an electric arc. As long as the only means of producing an electric current was by Volta's pile or battery with zinc plates,

it was not economical to employ this arc as an illuminant except for lecture purposes.

Not until after the dynamo machine had been perfected, in 1870, by which an electric current could be generated by revolving a bobbin of wire between the poles of a powerful magnet, did it become feasible to use the electric arc commercially, and then, owing to its brilliance, only for street and large building lighting. For years past it had been the object of numerous inventors to produce an electric lamp of small candle-power suitable for domestic electric lighting, which was called "dividing the electric light."

Various inventors, such as Edison, had tried mounting a thin wire of platinum, which is a very infusible metal, in a glass bulb and rendering it incandescent by an electric current so as to make a small electric lamp. But platinum melts at about 1700° and, like all pure metals, very suddenly if taken ever so little above that temperature.

Mr. J. W. Swan, of Newcastle, recognized that carbon or charcoal, which cannot be melted, would be better than any metal if a very thin rod of carbon or filament of it could be mounted in a glass bulb exhausted of its air, and held by platinum wires sealed through the glass so as to pass through it an electric current. He obtained some very thin rods of carbon, only

about one-sixteenth of an inch or less in diameter, and mounted one in a glass bulb as above described, and exhibited this carbon lamp in operation in Newcastle in December 1878. This lamp, subsequently called "Swan's Newcastle Lamp," is now in the National Science Museum at South Kensington, London.

About a year later Edison filed a British Patent Application for an incandescent lamp, comprising a glass bulb exhausted of its air, and having in it a carbon conductor made by the curious process of rolling out into thin rods a paste of lampblack or soot, mixed with tar, and then carbonizing this rod by heating it in an iron box filled with coke dust. But neither this "tar putty" lamp of Edison's nor Swan's "Newcastle" lamp were sufficiently practical to be of use to the public.

Swan, however, soon improved his invention by taking some ordinary cotton thread and dipping it in strong sulphuric acid, which converted it into a material like catgut, and then carbonizing a loop of this material by heating it in an iron box packed in coke dust. In this way he produced a fine thread of carbon in the form of a loop, which, when mounted in a glass bulb exhausted of its air, gave a light equal to about sixteen candles when an electric current was sent through the carbon thread.

About the same time Edison made a similar advance. He carbonized a thin strip of a Japanese bamboo bent into the shape of a hair-pin, and mounted this "carbon filament" in a glass bulb exhausted of its air, the ends of the loop being clipped into platinum wires sealed through the glass by which an electric current could be passed through the carbon filament, thus rendering it white-hot and giving out a light of about sixteen candle-power.

Thus about the year 1880 both Swan and Edison had solved in a perfectly practical manner the problem of "dividing the electric light," and discovered how to manufacture an electric lamp suitable for domestic electric lighting.

In the course of his work, Mr. Swan had made the acquaintance of Mr. C. H. Stearn of Birkenhead, who was a scientist of great experience in questions connected with the making of high vacua, that is, exhausting glass vessels very completely of their air. A company was soon formed at Newcastle to manufacture Swan lamps, and Mr. C. H. Gimmingham, who had been an assistant to Sir William Crookes, was appointed works manager.

Mr. Edison, however, had done more than merely invent an electric lamp. Adopting an idea due to Mr. Lane Fox, he proposed to establish in every town a central station for the supply

of electric current from dynamo machines driven by steam-engines producing electric current at a constant voltage. He devised a system of copper conductors enclosed in iron pipes, by which to distribute this current underground to houses and buildings, and he had worked out all the details of sockets for lamps, switches, electric meters, and fixtures for the supply of domestic electric lighting. Edison had also constructed large dynamo machines for giving electric current at constant voltage, and formed in the United States and Great Britain companies for carrying out his system.

In London the chairman of the Edison Electric Light Company was the Rt. Hon. E. P. Bouverie, and the secretary Mr. Arnold White, as in the case of the Telephone Company ; and I was appointed at the beginning of 1882 as the electrician, and Dr. John Hopkinson, an eminent scientific man and Senior Wrangler of his day at Cambridge, was scientific adviser. Then Major S. Flood Page, at this time the general manager of the Crystal Palace, arranged to hold a great Electrical Exhibition there, to show the public the then state of electric lighting. The different inventors, Edison, Swan, Maxim, Crookes, and many others, were invited to make displays of their systems.

The chief offices of the Edison Electric Light

Company were then at 57 Holborn Viaduct, and Mr. Edison's representative, Mr. E. H. Johnson, had erected in the basement two large dynamos (as we considered things in those days) of 150 horse-power, each to light the building and adjacent shops. There was a large elephant at that time in the Zoological Gardens which the children called "Jumbo," and these dynamos, considered colossal in those days, were called by the same name, Jumbo.

The Crystal Palace Exhibition, held in the spring of 1882, was a great success, and for weeks on end I was there nearly every day explaining the details of the Edison system to various parties of important people, and answering their questions. In those days Mr. E. H. Johnson used to expatiate on the perfect reliability of the Edison system, and that it could not possibly fail. Once at the Crystal Palace, when he was giving this comfortable assurance, the electric lights in the Edison Court all went out, owing to some failure in the steam-boilers !

The Gas Engineers held a meeting there to discover how far electric lighting was likely to prove a rival to gas as an illuminant. The conclusion at which they arrived was that, beautiful as it might be, it could not by any chance replace gas in domestic illumination. But it required a span of twenty years to falsify this prediction.

One of my first successes for the Company was to secure the contract for the electric lighting on one of the Admiralty's Indian troop-ships, the H.M.S. *Mooltan*.

Edison had devised and constructed a variety of small dynamos for private installations. But his design was far from perfect, and the bobbin or armature in which the electric current is generated when the bobbin is set in revolution had to revolve at a very high speed, about 1500 revolutions per minute, to create the required voltage of 110 volts for the Edison lamps. The only way in which this speed could be obtained was by driving the dynamo by a leather belt from the flywheel of a steam-engine. But this takes up considerable room.

In this first troop-ship the Admiralty allowed me to install a belt-driven dynamo. When, however, I went soon after to Glasgow to try to secure the contract for the electric lighting of a ship at the great shipbuilders, Messrs. Denny Brothers, they would not listen to a proposal for belt-driving, but said that the dynamo must be direct coupled to a high-speed steam-engine. The most compact high-speed steam-engine of that day was Mr. Peter Brotherhood's three-cylinder engine, but even the speed of that was not sufficient for direct coupling to the Edison dynamos.

Dr. John Hopkinson had, however, been considering the question, and had seen that the design of the Edison dynamo was faulty, and that by altering the shape of the magnet between the poles of which the bobbin revolved in which the electric current is generated, he could make the dynamo give the required voltage at a lower speed.

This Edison-Hopkinson dynamo was accordingly built, and, when coupled to a high-speed Brotherhood engine, made a suitable plant for ship-lighting, and enabled us to secure contracts.

The same modification was made by Colonel R. E. B. Crompton with a Willans engine and Gramme dynamo. Electric lighting for ships and public buildings could then proceed successfully, and projects began to be formulated for central station lighting on the plan of Lane Fox and Edison. But in Great Britain we always seem to be the victims of premature and unwise legislation. No sooner has Science given us some new appliance or invention, than our fussy politicians at once endeavour to tie it up with bandages of red tape and paralyse it by restrictions of Acts of Parliament. We have seen that this was done with Telephony, and it was also the case with Electric Lighting and with Electric Traction.

In 1882, when electric lighting was fairly launched on the world, the old bogie of

“monopolies” was again trotted out, and the Government of that day proceeded to pass an Act of Parliament which was ironically termed an Act to “facilitate” electric lighting, but the immediate effect was to make it impossible to secure the financial support necessary to develop public electric supply.

When I went over to the United States of America in July 1884, and explained the position to Mr. Edison, he said, “Why, they have throttled it!” And so they had. For six years the electric lighting industry in Great Britain was shackled by this 1882 Act, and only recovered its vigour and freedom when an Amending Act was passed, in 1888, which removed the disabilities, and enabled schemes for public electric supply to be undertaken and carried through.

Meanwhile two other great inventions had come into existence, namely, the storage battery and the alternating current transformer.

In the Edison system of electric supply there was no means of storing up electric energy so as to avoid night-shifts of work or to tide over any breakdown of the generating plant. In the early days of electric supply we used to consider it a great feat to run a large dynamo twenty-four hours on end, and in the small public supply from the original Edison offices

at 57 Holborn Viaduct we were constantly compelled to stop by reason of mishaps to the dynamos or electric mains.

As far back as 1859, a French chemist, M. Planté, had found that if two lead plates are placed in dilute sulphuric acid, and an electric current passed to and fro from plate to plate, with pauses between the reversal of current, one plate becomes covered with finely divided lead and the other with a purple powder which is called peroxide of lead or lead rust, and that the plates so prepared acted like the zinc and copper plates in a Volta cell.

The discovery did not become of practical importance until another French chemist, M. Faure, in 1881, discovered that this process could be immensely facilitated by putting on the plates an oxide of lead called litharge. The air we breathe contains a life-giving gas called oxygen, and without inhaling oxygen we could not live five minutes. The oxygen can combine chemically with metals, and the result is called an oxide of that metal. Thus the ordinary rust of iron is an oxide of iron, and the white powder we call magnesia is an oxide or rust of the bright silvery metal magnesium. The common metal lead had three oxides, called respectively litharge, red lead, and lead peroxide, which last is a purple or chocolate-coloured powder.

M. Faure found he could greatly improve on Planté's process and store up a large quantity of electric energy by the following construction. He placed in a box a lead plate covered with a layer of litharge, kept against it by a sheet of flannel. Then another layer of litharge and a second lead plate. The box was then filled with dilute sulphuric acid. If a current of electricity was sent for some time from one lead plate to the other through the acid, one lead plate became covered with a brown powder which was peroxide of lead and the other with a grey powder which was pure lead. Then if these two lead plates were connected by a wire the cell could give up again an electric current. It was called a storage cell.

Faure brought over from Paris to Glasgow such a box in 1881, containing a million foot-pounds of energy, to Sir William Thomson, afterwards Lord Kelvin, and he wrote an enthusiastic letter about it to *The Times*.

Faure's first lead oxide storage cell required, however, great improvement before it could be of practical use. For one thing, a better means was required for attaching the lead oxide to the lead plate. Among others, Sir Joseph Swan did much to improve it by suggesting the use of lead plates in the form of a grid, the holes being packed full of a cement made by mixing litharge

with dilute sulphuric acid. The practical perfection of these plates was also due to the work of the Electric Power Storage Company. In its final form it consisted of a glass or ebonite box, having in it a number of these "packed grid plates," the alternate plates being joined together by lead bars. The cell was then charged by sending an electric current through it for a certain time, and when charged to a voltage of 2 volts it could give back a current on discharge.

Sir William Thomson was then Professor of Physics in the University of Glasgow, and not only one of our most eminent scientific men but a great inventor as well. It was he who had invented the instruments which made commercial signalling through the Atlantic submarine telegraph cable possible. He had invented also a wonderful magnetic compass for ships and a method of sounding the depth of the sea by a piano wire. At the inception of practical electric lighting he recognized that it would be necessary to have instruments for measuring electric currents and electric voltages or pressures and electric power, and he began to design them.

On coming to London in 1882 I had been elected a member of the Society of Telegraph Engineers and Electricians, which title was in 1888 changed to that of "The Institution of

Electrical Engineers," and my position with the Edison Electric Light Company had shown me that not only would electric meters of various kinds be required to measure the electric current or power delivered to houses on the public supply circuits, but also some means of deciding if these instruments were correct.

In 1885 I read to the above-named institution a paper on "The Necessity for a National Standardizing Laboratory for Electrical Instruments." About fourteen of the chief members took part in the discussion on it, and entirely agreed with the Author. A committee was formed to consider what could be done to give practical form to my proposal.

A year or two later the Board of Trade established such a laboratory with the late Major Philip Cardew as its first superintendent, and it is generally agreed that this proposal of mine gave the first impetus to the movement which several years later resulted in the establishment of the National Physical Laboratory at Teddington.

This latter institution is not limited merely to testing or certifying the accuracy of electrical instruments, but has carried out on a large scale physical and engineering researches which would have been quite beyond the power of any individual worker or University laboratory to undertake.

I also began at that time to consider how we should construct standards of electric voltage and current and standards of light. This brought me into contact and friendship with Lord Kelvin, at that time Sir William Thomson, he having been knighted for his important share in laying the first Atlantic cable in 1866. He had not only scientific genius of the highest kind, but an unusual power of inventing practical appliances.

I was able to give him some facilities for obtaining large electric currents at Holborn Viaduct for testing his new electrical instruments. He invited me to visit him on his yacht, *The Lalla Rookh*, R.Y.S., and also at his house in Glasgow. He had been appointed a Professor in the University of Glasgow when only twenty-two years of age, such was his genius, and had then just graduated as Second Wrangler but as First Smith's Prizeman at Cambridge. For fifty years he gave to that chair at Glasgow a world-wide fame by his researches, inventions, and scientific investigations.

The chief Professors at Glasgow reside in houses round a quadrangle in the University precincts, and teach large classes of students in Scottish fashion by lectures for six months of the year. Lord Kelvin used to go across the quadrangle in term-time at 9 a.m. and give his lecture to about two hundred students. He never

seemed to prepare any special discourse, but give them extemporaneously the contents of his almost omniscient mind in physical matters. His lectures, however, were not so much information as inspiration, and the senior students of his class always felt that, with him as their instructor, they were in the presence of one whose vast genius for the exploration of physical phenomena lifted him quite out of the ranks of the ordinary teacher. That duty finished, he went to his experimental laboratory, where a few advanced students carried out his researches or made tests of instruments he was engaged in inventing.

During the years about 1885 to 1889, when he was working out and perfecting his electric measuring instruments, I had a constant correspondence with him, and still retain many letters received from him. When he was engaged on his scientific researches he almost forgot the lapse of time. He had a telephone wire run from his house to his laboratory, so that Lady Thomson could call him up when lunch or tea was waiting. In his dining-room he had a grey parrot, called Dr. Redtail, with remarkable powers of speech, and some one had taught this bird to say "Late again, Sir William," when he came into the room. When lunch or dinner was in progress, the parrot said nothing until the sweets came on to the table, and then in a

loud and raucous voice he used to say "Remember poor Poll." Sir William then got up, and gave him a bit of cake or tart.

Lord Kelvin appeared to have the unique power of carrying on two trains of thought, or attending to two things at a time. At meals or in company his eyes at times had an abstracted or far-away look as if his mind was "voyaging o'er strange seas of thought alone," but yet he could still attend apparently to what was going on around him.

At lunch I remember one day when his mind had apparently been pondering some abstruse scientific question, and Lady Thomson had been discussing plans for an afternoon excursion, Sir William suddenly looked up and said, "At what time does the dissipation of energy begin?"

Returning now to the story of electric lighting development in 1883, the Edison Electric Light Company and Swan Electric Light Company decided to amalgamate into one company, and transfer the Swan Lamp factory from Newcastle to a place near London. This was done to avoid contests about patent priority, and to facilitate proceeding against infringers. The lamp factory was moved to a site at Ponders End, near Tottenham. This change gave me the opportunity of forming the acquaintance of Mr. (later Sir) Joseph Swan and his partner, Mr. C. H. Stearn.

Mr. Swan was then engaged in finding a better material than cotton thread for the filament of his lamp, and he had devised a process closely similar to that used now in the production of artificial silk. Cotton wool was dissolved in some medium, and squirted out of fine holes just as a spider makes his thread. This produced a very uniform material, which when carbonized gave a uniform carbon filament. Before many years had passed, the material carbon was replaced by a wire made of the infusible metal tungsten.

In the summer of 1884 I obtained leave from my Directors to pay a visit to America, with the chief object of seeing how far electric lighting had progressed in the United States. I crossed the Atlantic for the first time in a vessel called the *City of Chester*.

The British Association had arranged to hold their meeting that year in Montreal, in Canada, and I therefore made my plans to be at Montreal at that time. My brother Howard was at that time settled in Plainfield, New Jersey, and had a business in New York City. I visited Boston, Philadelphia, and the Hudson district, Lake George, and went up to Toronto, also the Great Lakes and the Falls of Niagara ; and entering Canada, had an interesting week at Montreal, with a day's visit to Quebec and the

Heights of Abraham, where in 1789 Wolfe's great victory over the French under Montcalm gave Quebec and Canada to the British Empire.

Returning to New York, I examined carefully the working of the Edison Central Station system. Edison had selected a site on lower New York and built an electric station equipped with six of his "Jumbo" dynamos direct driven by Corliss engines. He had laid down a system of underground mains, consisting each of a pair of insulated copper conductors in an iron pipe, and was supplying electricity by meter to an area in the business district of New York. I obtained full details of all the capital outlay and working expenses and receipts, and on the way back to England I wrote a report for the directors of the Edison Company of London, showing that the public supply of electric energy for illumination purposes could be made a profitable industry.

On return I received before long an invitation from University College, London, to occupy the Chair of Electrical Engineering which the Council had a desire to establish, as it appeared the time had come to supplement the teaching of Civil and Mechanical Engineering by instruction in the principles of the younger branch of Electrical Engineering.

CHAPTER V

UNIVERSITY PROFESSOR AND ELECTRICAL CONSULTANT

FROM its earliest days there had always been lectures on Civil Engineering at University College, London, and some years before my appointment the late Sir Alexander Kennedy had established a mechanical engineering laboratory there for instructing students in the testing of materials and other experimental work in connection with Mechanical Engineering. The accommodation was, however, very small.

When I was appointed as Professor of Electrical Engineering, all that could be done at first was to provide me with a piece of chalk and a black-board. A year later I obtained a grant of £150, sufficient to give me a small gas-engine and a storage battery of twenty-five cells, with one small room as lecture-room and another as working laboratory.

I constructed, however, a photometer for testing electric lamps and provided a few

necessary electric-measuring instruments, and began to give lectures on the principles of electrical engineering, and took my students to visit various electrical works.

After about 1888 or 1889 a large number of towns began to erect electric supply stations or take over small private stations and enlarge them. This required in many cases expert advice, and some small amount of this work fell to me. In addition to this, the Edison-Swan Company began to have lawsuits with several firms or persons who were making carbon filament electric lamps and infringing the patents of Edison and Swan.

In Great Britain when a person makes an invention he can, if he chooses, obtain what is called Provisional Patent protection for it. In return for the payment of certain fees and formalities, the Crown grants him the sole right to make and sell his invention for fourteen years. He has to describe exactly how he carries it out or makes his invention, in a document called the Complete Patent Specification, and he must make definite claims at the end to describe precisely what is new. In order that his Patent may be valid, his invention must possess three qualities : It must describe something new—that is, it must not have been used, made, or described previously. In the second

place, it must have practical utility. A mere idea or scientific principle cannot be patented. In the third place, the Specification must have correct technical form. In the Provisional Specification the nature of the invention must be clearly foreshadowed or described in outline. In the Complete Specification it must be so described in detail that any person skilled in the art can carry it out without further instructions. Then the claims must precisely define the new and useful invention, and nothing else. In Great Britain a single invalid claim makes the whole patent void.

In a large number of cases the dispute about a patent turns on whether the patentee has "sufficiently ascertained" his invention or given such description that it can be carried out by any person fairly skilled in the subject. Edison obtained in 1879 a British Patent for a "carbon filament electric lamp of high resistance," but he did not define what he meant by a "filament." Strictly speaking, it means something as fine as a hair. But Edison did not say whether it was a matter of absolute size or merely relative proportions of length to diameter.

It is sometimes the case that information necessary for the performance of the patented process or thing is deliberately withheld, but if this is detected the patent is invalid.

I remember hearing of an instance in which a workman in some factory made a notable improvement in a manufacturing process, and communicated it to his employer verbally. The employer resolved to have it protected by patent, and sent the workman to his patent agent. When the patent was granted, and the Complete Specification was published, the employer was struck when he noticed that its contents differed considerably from the verbal description given to him by his workman. So he sent for the workman, and said, "How is this? The description in this Specification does not agree with the description you gave me privately of the way you carry out this improvement." The workman said, "No, I was not such a fool as to tell that chap (the Patent Agent) exactly how I did it."

The employer's disgust may well be imagined, because that confession of the workman made the patent void and worthless, and all the fees spent in obtaining it were wasted. Nevertheless, in some chemical patents there have been found some little necessary information ingeniously withheld or so concealed as not to be easily obvious.

After some preliminary litigation with smaller firms the great contest about the Edison electric lamp patents came on in 1888 with the Brush

Electric Lighting Company in the High Court of Justice, London, before Mr. Justice Kay. The attack of our opponents was directed against the sufficiency of the Specification, and that the so-called "tar-putty filament" of Edison could not be made, and was not practical. The Edison-Swan Company had as their chief expert witness the notable engineer, Sir Frederick Bramwell, with vast experience as a scientific witness. He was celebrated for a certain dry humour, and he had an impressive personality. As a speaker, whether at a public dinner or scientific function, he was unsurpassed.

Attending once at a dinner at Peterhouse College, Cambridge, to celebrate its six-hundredth anniversary, he is said to have been called upon at about 11 p.m. to propose the toast of "Applied Science." Rising slowly, he said that "at that hour of the night the only application of science which seemed to him at all relevant would be the application of the domestic lucifer to the bedroom candle," and he sat down. The American Ambassador wrote on the back of his menu card and pushed across to him the impromptu lines :

" O wise Sir Frederick !
Would that all could catch
Your charming brevity,
And find your match."

He was in urgent demand in scientific patent

lawsuits as an expert witness. It is said that when he was elected a Fellow of the Royal Society his professional fee was increased, and when asked why, he said, "F.R.S. meant Fees raised since." When he received the honour of Knighthood they were raised again, and when asked why, he said the "Nightwork was always paid double rate."

The Edison-Swan Company had also Dr. John Hopkinson, Sir James Dewar (the eminent chemist), and Dr. Tidy as witnesses, as well as the present writer. Owing to the contradictory scientific evidence given in the witness-box, the Judge declared that he must have the instructions in the Edison Specification carried out in the presence of an independent assessor, and he appointed Sir George G. Stokes, then President of the Royal Society, to be the assessor. All the Counsel, witnesses, and experts then adjourned to the Ponders End Lamp Factory, and ordinary workmen were told what to do and left to do it, making electric lamps with tar-putty filaments, and then working them with an electric current. The lamps were duly made, and operated for a dozen hours or so quite effectually as illuminants.

Thus the sufficiency of the Edison patent specification was proved. In spite of this, Mr. Justice Kay gave his judgment against the patent. The case was, however, carried to the Court of

Appeal, and they upheld the patent and reversed the judgment of the Lower Court.

After that a little more litigation gave to the Edison-Swan Company an absolute monopoly for the manufacture of carbon filament lamps for the few remaining years of the Edison patent. Nevertheless, carbon had great disadvantages. The evaporation of the carbon caused a blackening of the glass bulb inside and the "life" of the filament was rather short, and the efficiency or light given per unit of electric power spent on the filament was small. Not one-thirtieth of that power actually came out in the form of light.

Hence many inventors directed their attention to the discovery of a better material than carbon, and finally after much research the present-day tungsten wire filament was invented. Tungsten is a metal which has a very high melting-point, and can only just be melted in an electric arc furnace, which is the very hottest place we can produce on this earth.

Further difficulties were encountered before it could be drawn into fine wire suitable for the filament of an electric lamp. Later, it was found that after exhausting the glass bulb of its air, small amount of the rare gas Argon introduced into the bulb prevented the evaporation of the metal and gave us the highly efficient gas-filled

incandescent electric lamp of the present day, which is six or eight times more efficient as a light-giver than the old carbon filament lamp.

I shall, however, mention presently how the study of the carbon lamp led me to a very important invention in connection with wireless telegraphy. Meanwhile, it will be necessary to make reference to the important invention of the Alternating Current Transformer, which was done about the year 1883.

One of Faraday's greatest discoveries was that when an electric current begins or stops in a wire another brief or short electric current is induced or generated in another wire laid close to the first. If, then, the electric current in the first or primary wire is an alternating current, that is, one which continually changes its direction or flows to and fro, then the current in the second adjacent wire is also an alternating current. But then it must be noticed that when an electric current flows in a wire, the power being expended to make it flow is measured by the product of three things, namely, the current which is measured in "amperes," and the electric pressure at the ends of the wire measured in "volts," and a third quantity in 1892 to which I gave the name of the "power factor." These terms, amperes and volts, are the names of the units in which we measure electric current and

pressure, just as we use feet or yards to measure lengths and ounces or pounds to measure weight.

When we then cause a current in one wire to induce a current in another wire there is a certain small loss of power, but we can, by suitably choosing the length and size of the wires, cause a large current in one wire flowing under a small pressure to create a smaller current in the other wire at a larger pressure. Hence we can increase voltage at the expense of amperes, or *vice versa*.

If these two wires are wound round a core or bundle of iron wires or strips, this appliance is called a "Transformer," because it enables us to transform electric power from one state to another, namely, to create a large current from a small one by reducing the voltage or pressure from a large one to a smaller value. Although the induction coil and alternate current transformer were not new instruments in 1885, yet the right way of constructing and using them had not been previously found out. To transmit large amounts of power electrically to a distance, without wasting much of it in the conducting wires, we must either use very thick copper conductors, which means expense and weight, or else transmit small electric currents under high electric pressure. But the ordinary incandescent electric lamps are made to work with a pressure

of 100 to 250 volts, and are arranged or connected each across between two wires called the "mains." Hence to supply a large number of lamps in houses we need to provide a large current at a low voltage, whilst to transmit that power economically we need to provide a small current at a high voltage. This, then, is where the transformer comes in. It enables us to make this necessary transformation in the energy.

In 1885 there was an Exhibition at South Kensington, London, called the "Inventions Exhibition," and a Hungarian engineer, Mr. Zipernowsky, brought to England a transformer shaped like a life-buoy. It consisted of a ring of iron wire wound over with two cotton-covered copper wires, one a long thin wire, and the other a short thick one. He also brought a machine called an alternator, which could be revolved by a steam-engine, and created a current of electricity flowing backwards and forwards, and changing its direction fifty times a second. With these two appliances he showed that energy for electric-lighting purposes could be conveyed to a great distance on thin wires with small energy loss. I made some tests at the Exhibition, and was satisfied that Zipernowsky's claims were quite correct.

About the same time another very clever young

engineer, Mr. S. Z. de Ferranti, had arrived at the same conclusions, namely, that for the distribution of electric power over large areas the electricity must be generated and transmitted at high pressure or voltage and transformed down for use to a low voltage. Ferranti had invented, when quite a young man, an electric meter for measuring the electric energy supplied to a house, and charging for it as gas is charged for by meter.

Some gentlemen, chief among whom was Sir Coutts Lindsay, had established a picture gallery in Bond Street, London, and a small electric station for supplying it and shops near by with electric current for electric lighting. It was called the Grosvenor Gallery. Ferranti was a man with large ideas and great foresight, and he persuaded Sir Coutts Lindsay, Lord Wantage, the Earl of Crawford, and others to embark on a scheme of supplying electric energy for a large district of the West End of London from a supply station at Deptford, seven miles away on the banks of the river Thames.

This scheme began to be carried out about 1896. Ferranti designed immense alternators for generating electric current, reversing its direction twice 83 times a second, and generating at a pressure of 10,000 volts, or 100 times greater than Edison had used. Also he designed

transformers and underground cables for conveying the current at this enormous pressure to London.

When, however, the machinery was set in operation numerous accidents to the mains and breakdown of the transformers occurred. It looked as if the scheme was unworkable.

Then the original pioneers invited Mr. James Forbes, who was the chairman of the Edison-Swan Company, to be their chairman, and he invited me to be his scientific adviser and find out what was the cause of all these accidents.

After a good deal of thought and experiment, it became clear that Ferranti had overlooked that electricity flowing in long circuits has a tendency to run on and cannot be stopped instantly. It is like a motor-car or railway train or any heavy body in motion. Also at this high pressure of 10,000 volts electricity tends to jump across the air, and form electric arcs of great length. These blazing arcs, several feet long, often started on the switchboard when the long electric mains were suddenly switched in or out. I tried many experiments with transformers, invented by Ferranti and others, and in 1892 I read a long paper to the Institution of Electrical Engineers on these experimental researches on transformers, which excited great interest. In this paper I explained some of the reasons for

the failure of the transformers in the Ferranti system.

I also gave the results of an enormous number of measurements made by different methods on various kinds of transformers. At that time there was a great difference of opinion as to the relative advantages of transformers made with a bundle of straight iron wires spread out at the ends, called a "Hedgehog" transformer, and one made with a complete iron ring of wire or thin sheets of iron. The inventor of the "Hedgehog" was Mr. James Swinburne, a scientific man of great eminence, and he provided at first an instrument for measuring the power wasted in his transformer called a Wattmeter. But when I came to examine it carefully, I found that, wonderful as it seemed, his instrument measured too little when applied to his own transformer, but quite correctly when applied to a ring core transformer.

As an outcome of this research of mine, certain modifications were introduced into the working of the Ferranti Station at Deptford, which removed the chief cause of all the early trouble.

In addition to acting as scientific adviser to the London Electric Supply Corporation, which was the name given to the Company owning the Deptford Station, I was also requested to act for some time in a similar capacity to the City of

London Electric Company, of which my friend, Sir David Salomons, was the chairman. My consulting work was also extended to cities other than London. In Exeter and Taunton small electric supply stations had been put up by private persons, and the Corporations of these towns desired to buy up and extend these installations. I was engaged to value these stations and report on the required extensions.

Also the Town Councils of Peterborough, Plymouth, and of Douglas, Isle of Man, retained me to advise on the design of electric supply stations for their cities. This consultative work was not, however, very congenial to me. My tastes and predilections were rather for teaching and scientific research work.

Returning, then, to my work at University College, it may be noted that when Sir Alexander Kennedy resigned his position as Professor of Mechanical Engineering, he was followed in that chair by Professor (now Sir) Thomas Hudson Beare, and about 1892 he and I began to urge on our College Committee the absolute necessity for building and equipping new laboratories for mechanical and electrical engineering, to keep the provisions for those subjects at Gower Street on a par with those at other University Colleges. They agreed to complete the building of the south wing facing Gower Street for this purpose.

Our Professor of Architecture, Roger Smith, F.R.I.B.A., was the architect, and he followed in his design all our wishes as to the rooms and accommodation. To assist in their equipment I published a circular, and in a few months I collected about £800 for apparatus. Dr. Ludwig Mond afforded us generous assistance by the gift of electrical machinery.

These new laboratories were opened on May 29, 1893, by H.R.H. the Duke of Connaught, in the presence of a very distinguished company. The President of the College was then Mr. Erichsen, an eminent surgeon, and the Vice-President, Lord Reay. The laboratories were opened for the reception of students in September 1893, and during all that autumn I was kept extremely busy in organizing the teaching work. I had as my chief assistant Mr. W. C. Clinton, who rendered me and the College invaluable service. He had unusual capacity as a teacher, and in due time was made Assistant Professor, and to my great satisfaction he succeeded me in the chair of Electrical Engineering when I retired in 1926. With his aid the equipment and teaching system was carefully arranged and extended.

I wrote some hundred or more Laboratory Notes and Forms, each giving instructions for some particular piece of laboratory work for

a group of two or three students. These proved useful to many other teachers, and had for a time a world-wide circulation. For years we had nearly fifty students in the Department doing practical work, and Mr. Clinton and I had a very laborious time. Many of our students of the early days have since attained fame. One of them, Sir Joseph Petavel, F.R.S., is the present Director of the National Physical Laboratory. Three or four of them, Mr. Chattock, Mr. Edgecumbe, Mr. Roger Smith, and Mr. F. Gill, have been Presidents of the Institution of Electrical Engineers, London. Several of them, such as Mr. S. G. Brown and Mr. Edgecumbe, have been eminent as inventors, and many others have been 1851 Exhibitioners or Salomons Scholars, and taken distinguished honours in degree examinations. I always had some advanced students working at research work or for theses for higher degrees. I used to give by request special courses of Advanced Lectures available to practical men who were not regular students in the College. Some of these attracted large audiences of specialists, such as members of the Engineering Department of the General Post Office and wireless engineers.

My most important piece of consulting work was my connection with Marconi's Wireless

Telegraph Company, which began in the autumn of 1899.

As a pupil of Maxwell I had for long taken an intense interest in his theory of electro-magnetic waves, and in the experimental proof given by Hertz in Germany in 1887 of the existence of these waves and their production by very rapid alternating electric currents. I had also followed very closely the important researches of Sir Oliver Lodge on the same subject, and had myself made apparatus for repeating Hertz's notable experiments.

Hence when, in 1896 or 1897, it began to be known that a young Italian inventor had succeeded in using electro-magnetic waves to operate telegraphic apparatus over distances of several miles without the use of inconnecting wires, and so invented a practical system of wireless telegraphy, my interest in the matter was enormously increased.

In June 1897 Sir William Preece, then Engineer-in-Chief to the General Post Office, who had for some time been employing one of the older methods of wireless telegraphy, gave a Friday evening lecture at the Royal Institution, London, on this subject, and showed one of the early transmitters and receivers of Marconi.

Marconi had improved an instrument called a coherer, invented by Sir Oliver Lodge and by

a French scientist, M. Branly, for detecting the presence of electric waves, and had made a notable addition by the use of long vertical wires attached to high masts which radiated and collected these waves. He had in this way produced the first really practical system of electric wave telegraphy, and brought it to England in 1896 with an introduction to Sir W. Preece.

After numerous public demonstrations of its success I had the pleasure of seeing it for the first time in April 1898. Mr. Marconi had established one station in the Isle of Wight, at Alum Bay, and another at Bournemouth, near the Pier. Happening to be at the latter place for a short holiday, I made a request of Mr. Marconi to be allowed to inspect it, and he very kindly gave me permission.

I do not even now forget my astonishment when I saw a telegraphic instrument begin to print down in the Morse Code of dot and dash the message "Compliments to Professor Fleming," which had found its way across twelve miles of sea and been picked up by an aerial wire 150 feet high attached to a mast in the garden of the house in which Mr. Marconi was then residing at Bournemouth. In the following year, at Easter 1898, Mr. Marconi had established a station at the South Fore-

land Lighthouse, near Dover, and another at Wimereux, near Boulogne, and begun to give demonstrations of cross-Channel wireless telegraphy and communication with the lightship on the Goodwin Sands. This for the first time startled and aroused public interest and the Press, and reporters came down from all the principal papers to interview it.

On April 3, 1899, I wrote a long letter to *The Times*, describing the results of my inspection and foretelling a great future for this new method of intercommunication. In that year it had been arranged that the British Association should meet at Dover in September 1899, and that year was also notable as the centenary of the great discovery of Volta of the voltaic battery for producing the electric current.

The British Association Committee had invited me to give the principal evening lecture to the members, and I chose as my subject "The Centenary of the Electric Current." Mr. Marconi intended to make a voyage to the United States to exhibit there his system of wireless telegraphy, but he kindly arranged that a special demonstration should be made at my lecture. Accordingly a mast and aerial wire was erected on the tower of the Town Hall, Dover, and arrangements were made to send wireless messages during my lecture to the

Goodwin Sands lightship, and to exchange greeting between the President of the British Association, Sir Michael Foster, and the President of the French Association, M. Brouardel, meeting at the same time at Boulogne.

The lecture was delivered to a crowded and most distinguished audience in the Town Hall on September 18, 1899, and all my experiments and demonstrations of the Marconi telegraphy were quite successful. I received many compliments and congratulations on it afterwards. Further reference is made to it in Chapter VII.

When Mr. Marconi returned from the United States in the autumn of that year, having demonstrated the possibility of his electric wave telegraphy over a distance of upwards of one hundred miles, he had fully made up his mind to attempt the great feat of bridging the Atlantic Ocean.

We did not know in those days anything about the possibility of long-distance wireless. It seemed clear, however, that considerable power would be required. The apparatus Marconi had used up to that time consisted only of an induction coil, capable of giving sparks 10 inches long, and excited by the current from twenty voltaic cells. I surmised from the first that it would be necessary to translate this purely laboratory apparatus into engineering plant, and that at least 25 horse-power would be required.

Accordingly, the Company formed to operate Mr. Marconi's inventions appointed me in the autumn of 1899 as their scientific adviser, and I was asked to consider the specification of the necessary machinery for attempting transatlantic wireless telegraphy.

The first question was the site for such a station, and finally a position in Cornwall at a place called Poldhu, about eight miles from Helston Station, near a good hotel, was selected.

I ordered for the Company a 25 horse-power oil-engine, and bought for them an alternator capable of giving an alternating current of electricity at a pressure of 2000 volts, and also specified the construction of some transformers to raise this pressure to 20,000 volts.

In those days the method adopted for creating the electric waves was by charging a so-called Leyden jar or condenser with electricity, and discharging it in the form of a spark between two metal balls which were connected by thick wires to the two plates of the condenser. A Leyden jar consists of a glass bottle, the inside and outside of which are covered with tinfoil. If a charge (say) of positive electricity is put on the inner tinfoil, and an equal charge of negative on the outside one, there is then a storage of electric energy. If the two coatings are connected by a thick wire, then the electric energy

in this condenser, as it is called, is liberated in the form of an electric current, which rushes to and fro in this wire with great rapidity, creating what is called an electric oscillation.

If this wire through which the condenser is discharged forms one single turn wound round a wood frame, and if this single turn has a second wire of many turns wound over it, the arrangement forms a transformer which was given the name of a "jigger." If one end of the second wire is connected to a metal plate sunk in the earth, and the other end to a long vertical aerial wire upheld on a mast, then when the condenser is discharged through the single wire turn of the jigger the electric waves are radiated from the Marconi aerial wire.

Every electric circuit in which electric oscillations can take place has a peculiar rate of vibration of its own at which these oscillations move. It is like the pendulum of a clock. We cannot make a pendulum swing to and fro at any rate we please, but when left to itself it swings backwards and forwards at a rate depending on the length of the pendulum, or, in the case of the electric oscillation, on the capacity of the condenser and the nature or form of the wire through which the oscillations take place. We call this rate of oscillation the natural frequency of the circuit, and we measure it in "kilocycles,"

that is, in the number of thousand complete oscillations per second. Thus a circuit in which a hundred thousand electric oscillations take place in a second is said to have a natural frequency of one hundred kilocycles.

Sir Oliver Lodge had shown in a fundamental Patent Specification in 1897 the necessity for "tuning" the transmitter and receiver to the same frequency or natural time of oscillation in the case of wireless telegraphy.

Mr. Marconi had found that to be effective the two circuits of his jigger must have the same natural frequency or be tuned together. Then very powerful oscillations are induced in the secondary circuit of the jigger by discharging a condenser through the primary circuit, the circuit having at some point a pair of metal balls slightly separated, which is called the spark-gap. Each time that the condenser is charged with electricity and discharged with a spark across the spark-balls it produces a group or train of a dozen or two electric oscillations in the connecting wire. These trains of oscillations may come at the rate of, say, fifty to one hundred per second, but the oscillations in each train may be at the rate of hundreds of thousands per second.

In translating this spark apparatus into an engineering plant many great difficulties had

to be overcome. As soon as the Marconi Company had acquired the necessary site for a station at Poldhu, the plans for the first building were drawn by me on my lecture-table at University College, London. At the same time, I devised a form of condenser of large capacity. It consisted of a number of wood boxes filled with linseed oil. In each box were placed a number of sheets of zinc separated by glass plates. Alternate plates of zinc were connected together, and the two sets formed the "coatings" of a sort of large Leyden jar. I also devised means of starting and stopping the trains of oscillations by a "key" so as to send the trains out in long or short groups to form the Morse Code signals in so-called "dash" and "dot," which in different combinations form the letters of the alphabet for telegraphic purposes. The full technical details are described in my book, *Fifty Years of Electricity* (Ilfic & Sons), to which readers desiring more information are referred.

When all this apparatus was ready and the station building complete, one of the Marconi Company's engineers, Mr. R. N. Vyvyan, went down to Poldhu to erect it. Much work and many alterations were necessary before it was complete.

Meanwhile, Mr. Marconi had designed the aerial wires, and his assistant, Mr. G. Kemp,

was instructed to erect a circle of about twenty high wood masts to support the aerial wires in the form of a cone. But in September 1901 a great storm wrecked a large number of these masts, though, fortunately, the station and machinery were not injured. Nevertheless, with characteristic perseverance, Mr. Marconi re-erected a couple of the masts to support a fan-shaped set of aerial wires to attempt signalling across the Atlantic.

In the engineering arrangements I had made in the Poldhu Station there were two condensers, one of large capacity and one smaller, and two spark-gaps. The plan was to create one set of oscillations with the large condenser, and use these to charge the smaller condenser. By this method it was hoped to obtain more powerful high-frequency oscillations to charge the aerial wires than by a single condenser system.

Before success could be obtained, Mr. Marconi had to redesign the jiggers I had made, and to syntonize or tune together all the different circuits. The final arrangements are described in my book, *Fifty Years of Electricity*, and in a book by Mr. R. N. Vyvyan called *Wireless over Thirty Years* (George Routledge & Sons). This being done, he sailed for Newfoundland with two assistants, Mr. Kemp and Mr. Paget, on Novem-

ber 27, 1901, taking kites and balloons to elevate a temporary receiving aerial wire, and instructing his staff at Poldhu on certain days and times to send out groups of three short signals in the Morse Code, denoted by . . . , which signifies the letter S.

I have a recollection I had been promised a cable or telegram when signalling was to begin, but this promise seems to have been forgotten at the crucial time.

On early days in December, about the end of the second week in December, the balloon and kite were elevated, but the balloon was carried away with the strong wind. On Thursday, December 12, Mr. Marconi and his assistants, using a form of coherer, called then the Italian Navy coherer, were able to hear faint sounds indicating the letter S being transmitted across the Atlantic by electric waves sent out from Poldhu. I was, however, left in ignorance of this success until I opened the *Daily Mail* newspaper in London on the morning of Monday, December 16, 1901. In the newspaper jubilations and congratulations which followed this achievement, my name was not mentioned, as far as I could learn, in connection with this success at the time, but Mr. Marconi made adequate acknowledgments later. This achievement of sending electrical waves an eighth part

of the way round the world was considered by some scientific men to require explanation. We had no knowledge then why it did take place. It was not until much later that we understood that the upper regions of our earth's atmosphere are "populated" with particles of electricity, which make it a conductor of electricity.

This layer of electrified air at a height of sixty miles or so keeps the electric waves from escaping, and guides them round the earth. From the name of the one (Mr. Oliver Heaviside) who first drew attention thereto, this layer of air is called the "Heaviside layer." Previously to our knowledge of this fact, we had no justification for believing that wireless waves could cross the Atlantic Ocean. Further details about the electrical state of our atmosphere are given in a later chapter.

CHAPTER VI

MY SCIENTIFIC RESEARCHES AND WIRELESS INVENTIONS

RETURNING, then, to my scientific work at University College, London, and elsewhere, it should first be mentioned that in 1896 Sir John Pender died. He was one of the great commercial pioneers and organizers of submarine cable telegraphy, and one of the original Directors of the first Atlantic Cable Company in 1858.

Toward the end of the year 1896 an influential Committee was formed, with the Marquis of Tweeddale as Chairman, to establish some memorial to Sir John Pender and collect funds for this purpose. I had private information that, if I pressed the matter, some part at least of this memorial might take the form of an endowment of the Electrical Engineering Department of University College, London. I brought the matter before our College Committee, and the claims of University College, London, to some assistance were put before the leading members

of the Pender Memorial Committee. The result was that, when in March 1897 some £7300 had been collected, the Memorial Committee decided that a sum of not less than £5000 should be placed in trust with the Council of University College, London, to form an endowment fund for the maintenance of the Electrical Laboratory of that College, on condition that the Council name the Laboratory "The Pender Laboratory" and the chair of Electrical Engineering "The Pender Chair."

The cheque of £5000 was handed over to Mr. R. B. Haldane, M.P., by Lord Tweeddale at a meeting held in the Botanical Theatre of the University College, London, on July 2, 1897, in the presence of a very distinguished gathering. Mr. Haldane and I returned thanks for the gift, and Lord Kelvin, to whom submarine telegraphy is so greatly indebted, also gave an address. Lord Kelvin expressed his hearty concurrence with the allocation of the bulk of the testimonial fund to endow the University College Electrical Engineering Laboratories.

After the meeting I showed many eminent people over the new electrical engineering laboratories.

In the Annual Report of the Committee of University College, London, issued in March

1897, the following paragraph occurs in reference to the above-mentioned endowment :

“The Council have the pleasure in recognizing that these very satisfactory resolutions in so far as they affect this College are largely due to the energy, tact, and devotion of Professor Fleming, who, putting personal considerations on one side, has worked with unremitting zeal on behalf of the Department under his care.”

It unfortunately happened that, at the time this benefaction was made, the interest on trustee stocks was very low, in fact, not much more than 2 per cent. Hence, when invested, this only yielded about £120 per annum. Nevertheless, even this was useful in the purchase of apparatus. There was also another small fund, called the De Molyens Fund, and with the help of this money and the grants already obtained I was able to equip the laboratory fairly well for teaching and research. My plan was always to enlist the help of senior students in pieces of original research, so as to make them feel they were taking part in the enlargement of knowledge.

Before I was appointed to the Professorship in University College, I had my attention and interest directed to the subject of electrical measurements. It has already been mentioned

that, when at Cambridge, I had invented an instrument for very exactly comparing what is called the electrical resistance of coils of wire, which was described to the Physical Society in December 1879.¹

It may be well to explain here that there are three things which have to be chiefly measured in electric work—current, voltage, and resistance.

When a liquid, say water, flows in a pipe, a certain number of gallons or pints of water will pass per second or per minute. This water flows under a certain pressure which can be measured in pounds per square inch. It is the same with electricity flowing through a wire: a certain quantity of electricity flows per second, and there is a certain electric pressure, or potential as it is called, between the ends of the wire and a certain relation between pressure and current called the resistance of the wire. These things have to be measured in terms of certain units, and the units are called after the names of certain celebrated electricians.

Thus the unit of current is called an ampere, after A. M. Ampère, who was a celebrated French electrician. The unit of pressure is called a volt, after A. Volta, the Italian inventor of the battery; and the unit of resistance is

¹ "On a New Form of Resistance-Balance for comparing Standard Coils," *The Philosophical Magazine*, February 1880.

called an ohm, after G. S. Ohm, a German Scientist. It is necessary, then, to have actual standards of these units. It is the same with our measures of length and weight. At the Standards Office there is a bar which is the standard yard of Great Britain, and all yard measures of drapers or foot rules of carpenters have to conform to this standard. Also there is a standard pound in the form of a certain mass of metal, and all pound weights used by grocers or butchers have to be equal to this standard. In electricity, therefore, we need to make similar standards.

There is a certain form of voltaic cell, first made by Mr. Latimer Clark, called a Clark cell, and it has been agreed that at a temperature of 15° Centigrade, the electro-motive force or pressure of this cell shall be called 1.434 volts, the volt being the unit of electric pressure.

In 1885 I read to the Physical Society a paper in which I described another form of cell adapted for giving a very constant pressure or electro-motive force. It was a modification of a voltaic cell devised by Professor Daniell, of King's College, London. Also in the same paper I described an instrument called a potentiometer, adapted for comparing the pressure of different kinds of voltaic cell and obtaining the value directly in terms of a unit, called a volt. This

instrument proved to be of very great use in electrical engineering. Unfortunately I did not make this instrument in such a form as to be very portable or saleable, nor did I protect my ideas by any British patent application. The result was that other people began to make direct-reading potentiometers on my plan in various practical and convenient forms, and it is an instrument now to be found in every electrical laboratory.

In the same year, 1885, as already mentioned in Chapter IV., I read to the Institution of Electrical Engineers a paper in which I advocated the establishment of a National Laboratory for testing and certifying the accuracy of electrical instruments.

I foresaw that the introduction of electric supply stations would require houses to be provided with electric meters to measure the energy supplied in the form of an electric current, and that some independent authority would have to be created to certify to the consumers or public the accuracy of these meters as in the case of gas meters. In addition to the measurement of electric quantities, there was the question of the measurement of light. An electric lamp is an appliance for converting electric energy into light. Unfortunately the best lamps do this very imperfectly, and the light they send

out is, so to speak, adulterated with a large quantity of radiation called "dark heat," which does not affect our eyes. On the other hand, the glow-worm and the fire-fly have discovered the secret of manufacturing light without any admixture of heat. They can do what we cannot yet do perfectly, namely, produce an entirely cool form of light. I therefore turned my attention at one time to the question of standards of light or lamps with which to compare others.

There were at that time various flame standards, as they are called. For instance, there was a certain kind of candle called a "Parliamentary candle," used by the gas companies for comparing gas jets. Also there were other standard lamps with or without wicks, burning liquids called pentane or amyl acetate. But these standards were either very unconstant or else required great precautions in use. The incandescent lamps then in use had filaments of carbon, and the evaporation of the carbon in time blackened the interior of the glass bulb and reduced the light given out. I found that by treating the carbon in a certain way, and then removing the filament from a small glass bulb and remounting it in a much larger glass bulb, I was able to make a standard lamp giving a very constant light when supplied with a certain fixed direct electric current.

This work was carried out in the Pender Laboratory in 1896, and in work at the Edison-Swan Lamp Factory at Ponders End, Middlesex. In the year 1903 I read a long paper to the Institution of Electrical Engineers on the "Photometry of Electric Lamps," in which many different forms of photometer were described for comparing the light of electric lamps with the standards of light, and especially with the above large bulb standard electric lamps. This paper received one of the highest rewards of the Institution, called the Institution Premium. The large bulb standards were also adopted by the National Physical Laboratory as light standards, and were also more particularly described by me in a paper to the British Association at the meeting in Cambridge in August 1904.

The researches on the alternate current transformer carried out by me in the year 1902 have already been mentioned. These required an enormous number of careful measurements made with high-pressure alternating electric currents, and therefore dangerous. In these I was most ably helped by my then assistant, Mr. Shields, and by two of my students, Messrs. J. and D. Morris, at University College. These researches helped to settle certain disputed questions about transformers, and were, I believe, of great use in the subsequent design of this most

important implement in electrical engineering. In another paper on "Some Effects of Alternating Current Flow in Long Cables," read in May 1901, I also investigated some peculiar and dangerous effects taking place in the long underground cables laid down by Ferranti for the London Electric Supply which had been previously much discussed by electrical engineers.

I may mention next some of the investigations carried out over many years which finally led me to the invention of the Thermionic Valve used in Wireless Telegraphy and Telephony.

Quite early in my connection with the Edison Electric Light Company as scientific adviser, which began in the spring of 1882, my attention was drawn to a very remarkable thing in certain of the Edison carbon filament lamps made with a loop of carbonized Japanese bamboo mounted in a glass bulb exhausted of its air.

Carbon, which is the scientific name for pure charcoal, has the peculiarity that it cannot be melted to a liquid like metals. When the solid carbon is heated it passes at once into the form of vapour. This is the same with camphor, and also iodine.

If, then, the filament in an Edison lamp was too much heated, or "over-run," as we called it, by a too high voltage, the carbon evaporated and condensed on the inner side of the bulb and

blackened it uniformly, thus reducing the light emitted. But my attention was arrested by the fact that in a certain number of cases there was a white or clear line of no deposit on the glass which was in the plane of the loop. This showed me that the carbon atoms must have been shot off in straight lines from some one point on one side of the loop, and that the other side of the loop, so to speak, cast a "shadow," or, as I called it, a "molecular shadow," thus causing a clear line of no deposit on one side of the bulb.

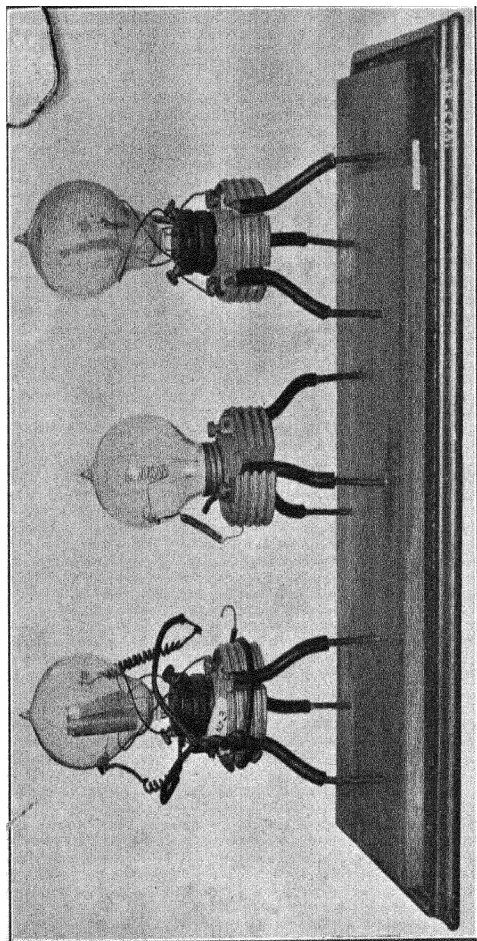
I presented a short note to the Physical Society on this effect, and a longer paper in August 1885. Just before that time, in 1884, Mr. Edison had made a remarkable observation on his carbon lamps. He fixed in the bulb a small metal plate between the two legs of the carbon hairpin loop. This plate was carried on the end of a platinum wire sealed air-tight through the wall of the bulb.

He found that when the filament was made incandescent by a direct electric current (that is, one flowing always in the same direction), a sensitive instrument for detecting electric currents joined between the wire carrying the above metal plate and the terminal of the filament at which the current illuminating the filament enters showed a small electric current; but if the connection was made to the terminal of the

filament at which the incandescing current leaves it, then no indication was given by the sensitive instrument. Edison gave no explanation of this effect, afterwards called the "Edison effect," nor did he make any application of it in telegraphy. He gave some lamps made with these inserted metal plates to Sir W. H. Preece, then the Engineer-in-chief of the General Post Office, and Preece read a paper in 1885 to the Royal Society of London on the subject, but did not give any explanation of the effect nor made any technical application of it. This effect, however, strongly interested me, and I felt it was worthy of more extensive examination.

In the next few years I spent a large amount of time in its investigation. In this I was assisted by the late Mr. Charles Gimmingham, who was the works manager at the Edison and Swan Lamp Factory at Ponders End. He made for me on my direction a large number of special lamps with metal plates or cylinders of various kinds in the bulb in various positions with respect to the carbon filament (see Plate I.).

Working at University College in my laboratory I then discovered two very important facts with respect to these lamps. In the first place; I found that from all parts of the incandescent filament particles of some kind were being given off which carried a charge of negative



PHOTOGRAPHS OF CARBON FILAMENT ELECTRIC LAMPS, WITH INSERTED METAL ANODE PLATES, AS USED BY SIR AMBROSE FLEMING FOR THE INVESTIGATION OF THE "EDISON EFFECT," AND AS RECTIFIERS OF HIGH FREQUENCY ELECTRIC OSCILLATIONS. The originals are in the National Science Museum, South Kensington, London.

electricity. If, then, any object charged with positive electricity was connected with the metal plate sealed into the bulb, that object was instantly discharged when the filament of the lamp was made incandescent by an electric current.

In other words, the hot-carbon filament was sending out torrents of negative electricity in the form of particles of some kind. In those days we did not know that electricity is made up of small particles which later on were proved to be far smaller than atoms of matter. These are now called electrons. I found that these particles could go through the holes or chinks in a metal grid, but could not go through a plate of glass or mica. I therefore assumed (erroneously) that these particles were atoms of carbon carrying a charge of negative electricity. The second discovery I made was that negative electricity from any battery could pass from the hot filament to the cold metal plate, but not in the opposite direction. The space between the filament and the metal plate sealed into the bulb is a one-way street for electricity.

I also discovered that the same facts held good for the arc lamp, but as the temperature is hot enough to melt any metal I had to use a thin carbon rod for my middle plate or third electrode. I described all these experiments in a lecture given on Friday evening, February 14, 1890, to a

very distinguished audience in the Royal Institution, London. This was the first occasion on which I had lectured at the table made famous by the discourses of Davy, Faraday, Tyndall, and Dewar, but to my great relief all my experiments went off well, and at the end the late (third) Lord Rayleigh said to me, "You have given us a great deal to think about." I made a much fuller account of them to the Physical Society on March 27, 1896, in a paper called "A Further Examination of the Edison Effect in Glow-Lamps." At the time when these researches were made, there was no practical application of them. They were purely scientific investigations.

Three years later, in 1899, as already mentioned, I became connected with Marconi's Wireless Telegraph Company in an advisory capacity.

In Marconi's early work he used as a detector of electric waves an improved form of the instrument Sir Oliver Lodge had called a coherer. Marconi's coherer consisted of a glass tube in which were two silver plugs separated by about an eighth part of an inch. Between these plugs was a very small quantity of finely-powdered metallic nickel mixed with a little silver. When this powder was loose it did not conduct a direct electric current, but if a feeble alternating current

of electricity was sent through it, the grains of metal were made to stick together, or cohere, and then they could pass or conduct a stronger direct electric current through them. Modifying some arrangements previously made by Lodge and a Russian named Popoff, Mr. Marconi constructed a simple apparatus which enabled him, by the aid of this coherer, to print down in short and long marks, called dots and dashes, on a paper tape signs indicating letters of the alphabet by means of trains of electric waves sent out from a long vertical wire as already described at a sending station, and pick them up at a receiving station, and by the rapid electric currents created in a receiving aerial wire by the incident electric waves, and detect them with his coherer.

The apparatus was quite practical, and served to initiate a real wireless telegraphy up to distances of 100 or 200 miles. It had the disadvantage that the coherer was easily upset by the stray electric waves called "atmospherics," which are produced mostly by distant thunderstorms and also by any nearby electric sparks.

After the success in transmitting the "S" signals across the Atlantic Ocean, Mr. Marconi invented a new type of receiver called a magnetic receiver, based on a discovery by Lord Rutherford that very rapid electric currents can knock the magnetic state out of an iron wire.

This magnetic detector was more certain in its action than the coherer, but it had the disadvantage that the signals could only be heard as sounds in a telephone, and could not be recorded on a tape. When the recording tape is used it is possible often to separate out false signals from alphabetic signals by a careful inspection subsequently. But that cannot be done when the signals are only audible sounds in a telephone. Hence there is a great advantage in having a permanent record of the wireless message.

In the case of submarine cables, Lord Kelvin (formerly Sir William Thomson) invented a beautiful instrument called a syphon recorder, which records on paper tape the messages in Morse Code signals, the "dot" being denoted by a little jerk of a pen to one side, and the "dash" by a jerk to the other side. But this instrument can only be worked with a direct electric current—that is, one flowing always in the same direction. The high-frequency electric currents created in the receiving aerial wire are, however, alternating currents—that is, they reverse their direction several thousand or several hundred thousand times a second.

The problem before me then was to discover how to change this feeble electric oscillation into a feeble direct current which could work the ordinary cable-recording instruments. There

were known devices for "rectifying," as it is called, low-frequency alternating currents, but nothing then known which would do it for rapid electric oscillations.

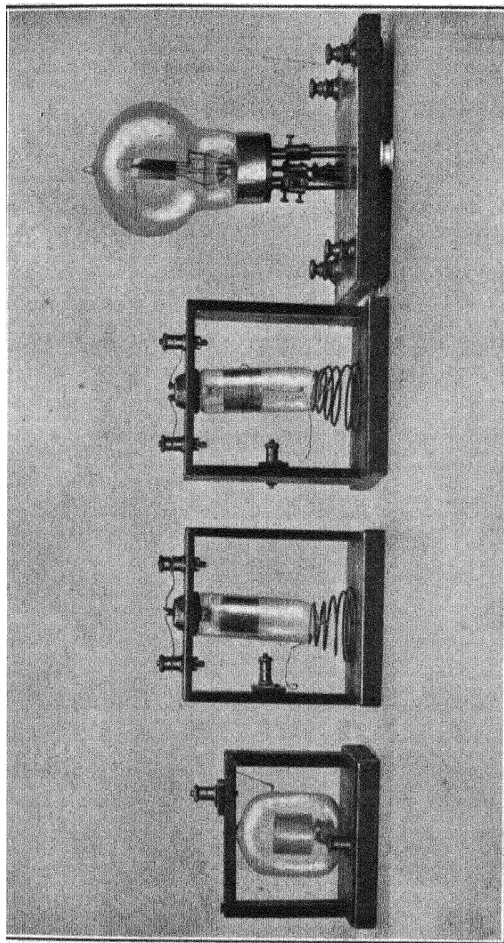
Thinking over the subject intensely, I had in October 1904 a sudden very happy thought. I recalled to mind my experiments on the "Edison Effect," and in particular my observation that the space between an incandescent carbon filament and a cold metal plate in a bulb exhausted of its air had a one-way conductivity for electricity. Then I said to myself, If that is the case, we have here the exact implement required to "rectify" high-frequency oscillations. I asked my assistant, Mr. G. B. Dyke, to put up the arrangements for creating feeble high-frequency currents in a circuit, and I took out of a cupboard one of my old experimental bulbs. The experiment was at once a great success, and I found I could use a mirror galvanometer, which is one of the instruments Lord Kelvin invented for submarine cable signalling, to detect feeble electric oscillations and denote their intensity by the movement of a small magnetic needle having a little mirror attached to it, and from that mirror a ray of light was reflected.

The next day I asked the manager of the Edison-Swan Lamp Factory to make me twelve carbon filament lamps, the filament of each being

rendered incandescent by a pressure of twelve volts. Round each loop filament I asked for a metal cylinder to be fixed carried on a wire sealed through the glass bulb, and I asked that the vacuum in the bulb should be as good as possible—that is, the air very perfectly exhausted.

When I received these bulbs I made the filaments incandescent by the current from six cells of a storage battery or accumulator. I then found that the space between the filament and cylinder would only allow negative electricity to pass from the filament to the cylinder, but not the other way. Hence the bulb acted like a valve in a pipe, which lets air or water only move in it one way. There is a valve in every motor tyre and motor pump, and there are valves in the veins of our bodies which allow the blood to move in them only towards the heart (see Plate II.).

Accordingly, I called this instrument a “valve.” At first I named it an oscillation valve, but before long it was usual to speak of the small particles of electricity sent out from a hot surface as “thermions,” and accordingly I called it a “Thermionic Valve.” This name has been very widely adopted, although it is often called a “Fleming Valve.” I regret to see that some authors prefer to denote it by the unmeaning name of “diode.” There seems to



PHOTOGRAPHS OF ORIGINAL THERMIONIC TWO-ELECTRODE VALVES AS FIRST MADE AND USED BY
SIR AMBROSE FLEMING, AS DETECTORS IN WIRELESS TELEGRAPHY IN NOVEMBER 1904.

be no advantage, quite the reverse, in coining a scientific gibberish to replace plain English short words for scientific apparatus.

My valve had the great advantage that it could not be injured or set out of action by "atmospherics," as is the case with the coherer. On November 16, 1904, I applied for a British Patent, No. 24,850, for this invention, and in due course obtained also patent protection in the United States of America and in Germany, which were granted, as well as patents in other countries.

This valve was the first instance of a detector for wireless waves making use of the emission of small particles of electricity, now called electrons, from a hot solid, and it proved to be the fertile parent of a series of very important and sensitive wave-detectors. It was very soon adopted, and used by Marconi's Wireless Telegraph Company as a practical detector.

In virtue of certain agreements I had made with Marconi's Wireless Telegraph Company in 1900 and in 1905, I was obliged to transfer to them these patents when obtained. Unfortunately my University work and other consulting and lecturing engagements prevented me from giving attention to further improvements in connection with my valve.

I had discovered in my experiments on the

Edison Effect that the stream of electric particles coming from the hot filament could be controlled by a magnet held outside the bulb. In one of my bulbs, which is now in the National Science Museum, London, I had placed a carbon filament and a zigzag wire of platinum with the object of discovering whether this wire with its apertures was as effective in catching the electrons as a solid plate of the same over all area. But, sad to say, it did not occur to me to place the metal plate and the zigzag wire in the same bulb and use an electron charge of positive or negative on the wire to control the electron current to the plate.

Three years after the date of my valve patent application, an American electrician, Lee de Forest, who had been following my work very closely, appreciated the advantage to be so gained, and made a valve in which a metal plate was fixed near a carbon filament in an exhausted bulb, and placed a zigzag wire, called a grid, between the plate and the filament. He thus put into one bulb the metal electrodes I had put into separate bulbs, and so made the first rudimentary form of so-called three-electrode valve, or triode, as some prefer to call it. He obtained United States and also British patents for this addition, but so little did he seem to value it that he allowed his British patent to lapse after

three years by non-payment of fees. He gave his modification of my valve the name of "Audion," and endeavoured as much as possible to claim for himself the entire invention and depreciate mine. This led a few years later to litigation in the United States.

An action was brought against the de Forest Radio Telephone and Telegraph Company and Lee de Forest by the Marconi Wireless Telegraph Company of America in the United States District Court of the Southern District of New York, and heard by the District Judge, Julius M. Mayer, who gave a very learned and exhaustive judgment on September 20, 1916, in favour of my invention, and found that de Forest's addition of the grid, though a contribution of value, was not independent of my prior fundamental invention.

The case was taken to the Court of Appeals, and heard by three Judges, and a unanimous decision was given by them in 1917, confirming the judgment of the Lower Court. The Appeal Court held that no one had anticipated my idea of rectifying high-frequency currents by an electron valve, and that my invention displayed "invention and of a very meritorious device" not in any way anticipated by the so-called "Edison Effect."

The question of priority of my invention also

came up in Great Britain on an application for the extension of my British patent. If a patentee considers he had not been able to obtain sufficient reward in fourteen years for his invention, the registered owner of the patent can make an application to a Judge in the High Court of Justice for an extension of the time. My valve patent expired in November 1918, and the registered owners, Marconi's Wireless Telegraph Company Limited, made an application for extension. This was strongly opposed by Government Departments and firms who had been making and selling my valves in enormous numbers during the Great War without licence or paying any royalties. The petition was heard by Mr. Justice Sargant in December 1918.

In the course of evidence it came out that the Marconi Company had made about £8000 by the sale of Fleming Thermionic Valves, and also sold U.S.A. and foreign patents for consideration. The Judge, therefore, held that no sufficient reason existed for an extension of the patent, and he dismissed the petition. He said, however, that my invention was a "pioneer invention of unusual utility," and as regards the three-electrode valve, although an improvement, he did not think it would ever have come into being but for the previous invention of the 1904 Fleming Valve.

This, then, rendered it impossible to recover any royalties on the millions of thermionic valves made during the War or before it. As the original inventor of it, I have never received a single penny of reward for it, other than by the retainer fee paid to me for years by Marconi's Wireless Telegraph Company. One thing I have always strongly resented, and that is the effort made by various manufacturers to disconnect my name with the invention.

One firm has sold valves for years made exactly in accordance with my patent specification, but which they advertise and mark as "Marconi Valves," though the Marquis Marconi had nothing to do with the invention. In the same way, other firms advertise "Cossor Rectifier Valves," "Phillips Valves," "Osram Valves," which are absolutely identical in principle with my legally certified valve, referred to by Mr. Justice Sargant as the "Fleming Valve." Such is the injustice of some present-day commercial practice.

Another of my wireless inventions to which I may briefly refer is that of the cymometer, or wave measurer. The term is derived from the Greek word "kuma," for a wave.

In all cases of wave motion we are much concerned with the wave-length. This term means the shortest distance from crest to crest of two successive waves or places of maximum amplitude.

Also the number of oscillations at any place per second is called the frequency. There is a simple relation between the frequency and the wave-length and the speed with which the wave travels. It is that the wave-length, multiplied by the frequency, gives the wave-velocity.

The velocity of wireless waves is the same as that of light, namely, 186,000 miles per second nearly, or 1000 million feet per second. If two electric circuits are put near each other, electric vibrations in one circuit will create vibrations in the second circuit. If these two circuits are tuned together, the effect will be the greatest possible. What I did was to construct a circuit, the natural frequency of which could be altered, and then it is put near any wireless transmitter and altered until the current in the testing circuit is a maximum. I employed a glass tube filled with the gas Neon—now used so much for shop signs, which glows with a brilliant red light when an electric current passes through it—to tell when the current was at its greatest.

I showed this instrument for the first time to a scientific audience at the meeting of the British Association at Cambridge in August 1904. A number of them were made by the Marconi Company, and this cymometer could be used for several other interesting measurements with high-frequency currents.

In the weekly paper, *Radio Times*, the wavelength proper to each station is given in metres. Thus Daventry National is 1500 metres. The frequency is given in kilocycles (k.c.) ; 1 kilocycle is 1000 vibrations per second. If we know the frequency in kilocycles, and divide this number into 300,000, it will give us the wavelength in metres. The frequency of Daventry National is 200 k.c. I described this cymometer in a paper to the Royal Society in 1904, about the same time that I gave the first published account of my thermionic valve.

During the next twenty years several scientific papers were communicated by me to the Physical Society, in which were described devices for measuring high-frequency electric currents as used in wireless telegraphy. I may, however, here make particular reference to a number of papers published jointly with Sir James Dewar. He was the Resident Professor at the Royal Institution, occupying the position formerly held by Faraday, a famous experimental chemist as well as a most kindly and popular man. He was the first to liquefy and obtain in quantities liquid air, liquid oxygen, and hydrogen.

This was achieved principally by his invention of the thermos flask. When he first succeeded in obtaining in quantities liquid air which

has a temperature of about 200° Centigrade below the freezing-point of water, I asked him one day if I might test at that temperature the electric resistance of a carbon filament. Carbon is peculiar in that its electric resistance decreases when it is heated, whereas in the case of metals it increases. That means that if we cool a metal wire it becomes a better conductor of electricity, but a cooled carbon filament would become a worse conductor. We agreed, therefore, to try some experiments together. But the trouble was to hold the liquid air long enough. It boiled away as quickly as water would in a red-hot saucepan. One day he gave me a sketch showing a sort of double-walled bottle with the air between the double side exhausted. I had it made for him at the Edison-Swan Lamp Factory, and afterwards his clever assistant, Mr. Lennox, made them at the Royal Institution.

This was the beginning of the famous thermos flask. In it could be kept liquid air in a quiescent condition for hours, not boiling away rapidly. This great invention made it possible to possess large quantities of liquid air, which looked like water, but was intensely cold, and to put into it various objects and discover the effect of cooling them to a temperature twice as far below the freezing-point of water as the boiling-point of water is above it.

I then prepared a number of small coils of wire of different metals, and Sir James Dewar and I carried out, about 1892-93, a large number of experiments on the effect of very low temperatures on the electric and magnetic properties of matter.

We published together about fifteen papers in a few years in the *Proceedings of the Royal Society*, describing the results. These papers were all actually written by me, and much of the work done by me and some of my students. I must particularly mention the great assistance of Sir Joseph Petavel, one of my old students at University College, London, who for many years past has been the Director of the National Physical Laboratory.

We found in our experiments that pure metals when cooled in liquid air became far better conductors of electricity, and it has since been found that in liquid hydrogen or liquid helium some metals become perfect conductors, which means that when an electric current is set going in them it would continue for ever.

We also made a large number of experiments on the effect of low temperature on insulators and on the magnetic properties of iron. These results were described by me in a Friday evening discourse at the Royal Institution on June 5, 1896, to a large and brilliant gathering of the members, with Lord Kelvin in the chair.

Altogether in the sixty years of my scientific work I have written and published something like one hundred scientific papers, not including numerous small contributions and articles for scientific journals. Most of these papers appeared in the *Proceedings of the Royal Society* and the *Physical Society of London*. The nearly complete list of them is given in my book, *Fifty Years of Electricity*. I read the first paper to the Physical Society in March 1874, and my most recent one in February 1932, or fifty-eight years later.

CHAPTER VII

POPULAR LECTURES AND SCIENTIFIC BOOKS

IN my scientific life I have been much occupied with public lectures both to popular audiences and to audiences of experts.

To succeed in this branch of work a man must have mastered to some degree the art of elocution. He must speak clearly and correctly, and not too slowly or too fast. This last-named fault is one which I have been told I myself have a tendency to commit. Then, if showing lantern slides, he must not turn his back on the audience, or talk in a conversational tone. Essentially he must have something to say which interests the majority of his hearers, or he must make up his mind which part of the audience, in a mixed one, he wishes to address, whether the uninitiated or the experts. And in the third place, if his lecture is an experimental lecture, the experiments must be a success.

No lecture can be attractive if the lecturer reads verbatim from a manuscript. My own

plan for a lecture of any importance is first to write it out fully just as it would have to be written for the Press. Then to prepare a list of the experiments or slides with a few brief notes under each item, and to write out an introductory paragraph and final concluding sentences, which are committed to memory. It is important to begin and end a lecture well, verbally and not in a slipshod way.

I also furnish my assistant or lantern operator with the list, and a few notes on it as to any special precautions. If there is a hitch in the operator's or assistant's work, the lecturer should cover it up by continuing to speak, and not make any awkward pause, far less scold, as some lecturers have done.

Also a touch of humour or an apposite story will put the lecturer on good terms with his audience. A notable popular lecturer, Sir Robert Ball, was very famous in this respect. Once coming on to the platform for a lecture a little late, he explained that the man at the door would not let him in, and when he said, "I am the lecturer, you must let me in," he said the man replied, "Oh yes, two or three have tried that trick already."

Another hint of importance is to be careful to explain as far as possible any words strange to a general audience. It is remarkable to

discover how many words, as familiar as their own names to science lecturers, are absolutely meaningless, or convey wrong meanings, to a popular audience. Once, during an Electrical Exhibition at the Crystal Palace, I was requested by the management to give one or two explanatory popular lectures in one of the large halls to the visitors to the Exhibition. One of these I gave was entitled "On Magnets and Electric Currents." I showed a familiar experiment of sprinkling iron filings on a sheet of glass, laid over a bar magnet which arrange themselves in beautiful curves called lines of force. I projected the image of this on the screen with an optical lantern. An old lady was asked afterwards if she liked the lecture, and what it was about. She replied, "Oh yes, it was very nice ; he talked about ' maggots ' [magnets] and showed ' creeping ' things [iron filings] on a sheet."

I did not, however, do much in the way of popular lecturing until after I had received my "baptism of fire" by being asked to give a Friday evening discourse at the Royal Institution, London, by Sir James Dewar, in February 1890. As already mentioned, the critical scientific audiences assembled there, who have heard Huxley, Tyndall, Frankland, Dewar, and other great notabilities of the nineteenth century,

demand a special competence in those permitted to address them.

The Lecture Theatre of the Royal Institution, London, is semicircular in form, with seats rising steeply above one another and a gallery above them. On the front of the gallery is a clock which strikes only one stroke loudly at the hour.

Every Friday evening during the winter half of the year, a discourse is given at 9 p.m. by some selected lecturer or eminent person. Owing to lectures on the previous days, the time available for arranging experiments or preparation on the lecture-table is limited. On arrival on the assigned Friday evening, the lecturer is shown into a small private room, where he waits with somewhat the same sensations of anxiety that we nearly all feel when shown into the dentist's waiting-room to await our turn to "take the chair" in the operating-room.

In the theatre a rope is fastened five rows of seats from the bottom, which are reserved for members and special guests, and the public admitted by ticket take their seats above it. At ten minutes to 9 p.m. the rope is withdrawn, and there is a scramble for vacant lower seats except the bottom row, reserved always for members of Council. These, with distinguished

guests, assemble in the library. At five minutes to 9 p.m. the eminent man selected as chairman leads in on his arm the principal lady, maybe the wife or some relative of the lecturer, and takes his seat on an arm-chair on a slightly raised platform in front of the lecture-table. He is followed quickly by members of Council.

Meanwhile, the lecturer has to wait outside one entrance door, near the lecture-table, until the theatre clock strikes one stroke at 9 p.m., when the door is thrown open and he has to step in to the lecture-table and make his bow in response to subdued or loud applause. It is etiquette to begin the subject at once without any preface, and to end up the lecture at the moment the clock strikes one stroke at 10 p.m. The audience then crowd down to the table to examine specimens or see experiments repeated. In my time it was the custom for Lady Dewar to hold a reception after the lecture in her drawing-room upstairs, in the apartments of the Resident Professor, Sir James Dewar, to which members of Council and eminent members of the Institution were welcomed, and hospitality was dispensed.

In all, I have given there eight Friday evening discourses in thirty years, between 1890 and 1920, at the table from which Davy, Faraday, and Tyndall have delivered their eloquent

addresses. The first of these was on "The Physics of an Electric Lamp," in 1890, in which I explained the discoveries mentioned in Chapter VI. The last was on the use of the "Thermionic Valve in Wireless Telegraphy and Telephony," in 1920.

In addition to the Friday evening discourses, there are at the Royal Institution courses of afternoon lectures, in series of two to five lectures. One such set, called the Tyndall Lectures, was founded in memory of Faraday's successor, John Tyndall. I have had the privilege of giving this course four times, and four other courses as well.

Furthermore, there are the familiar Christmas Lectures, which were initiated by Faraday and have been given for nearly one hundred years without break, in the fortnight immediately following Christmas Day. These comprise each six lectures given on alternate week-days.

I have had the honour of giving this course four times—in 1894-95, on "The Work of an Electric Current"; in 1901-2, on "Waves and Ripples in Water, Air, and Æther"; in 1917-18, during the War, on "Our Useful Servants, Magnetism and Electricity"; and in 1921-22, on "Electrons, Electric Waves, and Wireless Telephony." These Christmas Lectures are by a time-honoured custom sup-

posed to be addressed to a juvenile audience, but as a matter of fact there was always a very large mixture of adults in the audience. The last course of mine was particularly well attended, several hundred children being unable to obtain admission. It is remarkable that the youngsters often seemed to understand better what was being said than the adults.

In the last course above-mentioned I was at one point endeavouring to explain the mysteries of Wireless Telephony. One gentleman whispered to his little boy, "I hear what he (the lecturer) says, but I really don't understand it." His boy said to him, *sotto voce*, "I understand it all right, Father. You wait till we get home, and I'll explain it to you."

Very often I have been asked very intelligent questions by boys after such lectures. For instance, two quite small boys, after one of these lectures, asked me how the "earth connection" was made in the case of wireless in a flying aeroplane?

The course on "Waves and Ripples" was also very successful, and I had the pleasure at one lecture of showing an entirely new musical instrument, namely, Mr. Stroh's violin, which is a sort of combination of string and wind instrument. The Press notices of these lectures were very full and appreciative. These Royal

Institution lectures brought me many invitations to lecture at other places, and caused me for some years to be invited to give the Gilchrist Lectures.

The Gilchrist Educational Trust was founded in the last century by Dr. Gilchrist, to provide means for giving information on Science to popular audiences at merely nominal prices. The plan was to select five towns not too far apart, and appoint five lecturers of proved ability to give the same lecture at each town in succession, the lecturers changing about each night so that every one of the five towns had the five lectures. The lecturer's expenses and fees were paid by the Trust, and the people who attended paid a penny or two for their seats. These lectures were eagerly desired, and attended by large audiences in days before the popular taste was deteriorated by the type of films exhibited at many picture palaces at the present day.

I have myself lectured, forty years ago, to an audience of four thousand people in the Mile End Assembly Hall, London, extremely attentive and appreciative, and the same in other towns. My colleagues were often Sir Robert Ball, who talked on Astronomy in his inimitable way, and Dr. Dallinger, who was a naturalist, and gave a course on "Spiders,"

illustrated by wonderful moving slides in which the cruel craftiness of the common or garden spider in catching the common or house-fly was made manifest ; showing the way in which the poor little foolish fly was roped in by the spider's gummy thread, tied up wing and leg, and then slowly eaten alive in helpless captivity. Perhaps some critics would say that this was as bad as any modern film. At any rate, it did not encourage infantile yearning for crime as do some modern films.

A gentleman patted the curly head of a little boy in a shop one day, and said to him, "What are you going to be when you grow up, my little man?" and the boy promptly replied, "A burglar!" "That," said his mother, "is the effect of the pictures."

Between 1893 and 1896 I gave the Gilchrist Lectures in twenty towns in England. Besides this, I was called upon to lecture at about eighty places to local Scientific or Philosophical Societies, on engagements made for me by a lecture agency. When, for instance, any great scientific novelty came forth, there was in those days an immediate demand for popular lectures on the subject then attracting attention.

When Röntgen, in 1896, discovered the wonderful so-called X-rays, I remember Sir Joseph Swan said to me one day, "There is a

man in Germany who says he can see the bones inside the living human hand. Do you think that is possible?" In my ignorance I said I thought it was probably a newspaper exaggeration or invention. Yet a month later, to my rebuke, I was looking at the bones inside my own hand at a soirée of the Royal Society of London, by the aid of these Röntgen rays. I realized then that there would be a demand for lectures on it, and I provided myself with a Röntgen bulb and an induction coil, and various appliances for public discourses on this new discovery. So it proved to be, and I had soon many invitations to lecture upon it.

In some of these I was assisted by a young boy, who had a small bullet located in his hand through playing carelessly with a revolver. As it did not cause much trouble, the surgeons did not think it necessary to remove it. It enabled me to show, in quite early days, the use of Röntgen rays in locating metallic objects in the human body. During my lecture I used to make this boy place his hand on a photographic dry plate enclosed in a black paper envelope, and expose it for a few minutes to the X-rays. The plate was then taken into a dark room and developed by my assistant, and brought back and put into the optical lantern in the lecture-room, to show the audience the image of the

bullet and where it was embedded in the boy's hand.

I explained also how these rays could enable any one to distinguish at once between a real diamond, which is transparent to the X-rays, and the very best imitation diamond, which is opaque to these rays.

In explaining why they were called X-rays, I used to point out that X is the symbol employed generally in mathematics to denote the unknown quantity, the magnitude of which it is desired to determine. As Röntgen did not know the true nature of this radiation when he discovered it, he called it the X-rays. It was many years before the true nature of the rays was discovered.

Once in a lecture at Portsmouth, when explaining this use of the letter X as the unknown quantity, I added on the spur of the moment the suggestion that perhaps that was why so much beer was marked in the cask with double X (XX). A gentleman in the front row, who must have been an ardent total abstainer, nearly fell off his chair with laughter at this jibe.

Another institution at which I have given many lectures to expert or professional audiences is at the Royal Society of Arts, at John Street, Adelphi, London. This was also, like the Royal Institution, originally founded to promote the

technical applications of Science, and many important announcements have been made from its lecture-table. I have given there courses of Cantor Lectures eight times, in addition to several special lectures by invitation of Sir Henry Trueman Wood, or the present secretary, Mr. G. K. Menzies. One of these courses, on "Hertzian Wave Telegraphy," was given in the quite early days of wireless telegraphy, and attracted large audiences. At one of these Lord Kelvin was present, also the Marquis Marconi, to whose work we owe this method of telegraphy.

Of the two very special lectures to which reference may be made, one was given on November 23, 1921, with the late Mr. Alan A. Campbell Swinton in the chair, and was a lecture founded to commemorate the great services of Sir Henry Trueman Wood, as secretary for many years of the Royal Society of Arts. The subject was "The Coming-of-Age of long-distance Radio-Telegraphy, and some of its Scientific Problems." The lecture recounted, in the first place, the history of long-distance wireless telegraphy since the start of the first attempt of Marconi to signal across the Atlantic in 1901 by wireless waves. Reference was made in this lecture to the reasons which can now be given for the remarkable power of electric

waves to follow round the curvature of the earth.

Whilst the Poldhu Station was being prepared for a first attempt at transatlantic wireless transmission, I was accustomed to point out that between Poldhu and the coast of North America there was, so to speak, a mountain of land, covered with sea water, one hundred and fifty miles high, and the question was whether the electric waves could climb over it. Later on, mathematicians proved that merely in virtue of wave properties they could not do it, and that if our earth were destitute of any atmosphere like the moon long-distance wireless would be impossible, for the waves sent off from a station would soon glide off the earth altogether, and not curve round it. To make a long story short, the possibility of long-distance wireless telegraphy depends on the fact that the air is full of particles of electricity, electrons, or else chemical atoms which have lost electrons and become converted into positively electrified particles called "ions." These are produced by the action of sunlight on the air, and perhaps in some degree by the so-called cosmic rays.

The electrification of the air increases as we ascend up in it, and it can be shown that above a height of about twenty-five or thirty miles this "ionized" air can be roughly divided into

two parts. The lower part, which begins at about fifty to sixty miles high, is called the Heaviside layer, and the upper, beginning at about one hundred and fifty miles, is called the Appleton layer, named after the scientific men who first drew attention to their properties. These layers of electrified air act like a ceiling to the air, and keep the wireless waves from escaping by reflecting them down again. The longest waves, many thousands of feet long, are bent round by the lowest layer of ions about thirty miles high up. The medium waves are bent round the earth by the Heaviside layer, and the short ones by the Appleton layer, whilst some waves escape altogether. These escaping waves often travel for millions of miles, and are then reflected or bounced back to the earth probably by clouds of electrons or ions sent out by the sun.

One of the last, if not *the* last, lectures I delivered at the Royal Society of Arts was on May 20, 1925, on "William Sturgeon and the Electro-Magnet."

William Sturgeon was a self-taught genius, whose chief electrical inventions were made rather more than a hundred years ago. He was a shoemaker, who later enlisted in the Royal Artillery. His great invention was the electro-magnet. If a copper wire, wound over with

cotton or silk to insulate it, is wound spiral fashion on an iron bar, and if the wire is connected to a Volta battery for sending through it an electric current, the bar becomes temporarily (whilst the current flows) a powerful magnet. Every electric bell contains such an electro-magnet, and also every telegraphic instrument. It is the most potent weapon of the electrical engineer.

Sturgeon read an account of his invention to the Royal Society of Arts, and they gave him a silver medal for it. He left his original magnet with them, but so little was it valued that it was probably thrown away as junk. At any rate, it has vanished. From the drawing in his paper I made, however, an exact copy of the original electro-magnet, and showed it in my lecture, and afterwards bestowed it to the National Science Museum, South Kensington, where it is at present.

Sturgeon made many other very important electrical inventions, and was the first to amalgamate with mercury the zinc plates in a Volta cell, to preserve them from useless waste.

In this lecture I showed a good many experiments illustrating the wonderful magnetic properties of iron and its alloys ; and at the conclusion the chairman, Mr. Campbell Swinton, paid me a great and graceful compliment on the

constant success of my lecture experiments ; he said it had been a delightful historical lecture, finishing up with information on recent and valuable discoveries.

To conclude this part of my memories, I may refer again briefly to two lectures delivered to the British Association at Liverpool—one on September 19, 1896, on “The Earth a Great Magnet.” This was the so-called working-man’s lecture, which is always arranged for an audience of manual labourers or working-men. The chair was taken by the late Sir Frederick Bramwell. The other was addressed to the whole British Association Meeting at Dover, on September 18, 1899, on “The Centenary of the Electric Current.”

Both these lectures were fully illustrated by experiments, and were greatly appreciated. Knowing how much the life of Liverpool depended on shipping, and that ocean travel is entirely dependent on the Mariner’s Compass, I felt sure that it would interest my audience there to understand that the action of the compass wholly depends on the fact that the earth itself is a great magnet, and but for this all long-voyaging out of sight of land would be scarcely possible.

The second lecture was inspired by the fact, already mentioned, that 1899 was the centenary

of the invention by Volta of the means of producing the wonder-working electric current. This last lecture was attended by a brilliant audience. It cost me much labour to prepare, and I was intensely relieved that it all went off well. I hope I may be absolved from the charge of egotism if I quote a letter received next morning from my old colleague and friend, Major Flood Page, who was managing director for years of the Edison-Swan Electric Light Company, and for a short time of Marconi's Wireless Telegraph Company of London. It was as follows :

LORD WARDEN HOTEL, DOVER,
Sept. 19, 1899.

MY DEAR FLEMING,—I hope you have slept well and risen refreshed. With all my heart and soul I congratulate you on the most able, most interesting, and most brilliant lecture I ever heard in my life. Sir W. Crookes in no way exaggerated when he compared your lucid explanations of abstruse questions to Faraday. Words fail me to convey to you my sense, alike of your complete mastery of the entire subject and your most pleasant method of teaching and carrying your rapt audience with you.

One feeling animated the closely-packed assembly, which may be summed up as intense interest and admiration. Sir Frederick Bramwell's one word summed

it up admirably — “Splendid.” Sir W. Crookes said to me, “He told me many things I did not know.” A Liverpool merchant said, “I never thought of the clock till he had done.”

So it was with us all. That God may spare you long to continue your brilliant work, and that I may in the future as in the past pick up some crumbs which fall from your well-furnished mind, and ever retain your most pleasant friendship, prays your sincere friend,
S. FLOOD PAGE.

The local and daily Press were also complimentary. The *Daily News* of September 20, 1899, said :

The leading subject of conversation in the Reception-Room at the early muster was Professor Fleming's electrical experiments and lecture last evening. It is considered to be the most brilliant affair of the meeting, and full justice is paid to the expert assistants through whose skilful co-operation the numerous demonstrations, including the wireless telegraphy operations, were carried out for a couple of hours without a hitch.”

This last remark leads me to recall the names of one or two old students who have acted as lecture assistants to me with much efficiency in important lectures. First, I must mention Professor J. T. MacGregor-Morris, now Professor

of Electrical Engineering in the East London Technical College, and one of my earliest students in University College. He assisted me most ably in very numerous lectures, and always without the very slightest failure, and was an assistant in University College as well. Then next I call to mind the name of George Bewsey Dyke, a dear good fellow, who was my assistant in research and lecture work for eight years, and only left me to take a position in the works of Messrs. Siemens Brothers, where he would soon have taken a high position. He was called up in the Great War, enlisted in the Royal Garrison Artillery, and would assuredly have gained the Military Cross. But to the infinite sorrow of his relatives and friends and all who knew him he was killed by a German shell in April 1916. His was one of those valuable lives of young scientists sacrificed in that detestable War.

I have already mentioned the aid given to Sir James Dewar and myself in Low Temperature Research by Sir Joseph Petavel, also an old student of mine ; and also the work of my assistant, Mr. Shields. Probably the most important lecture I have been called upon to give to an audience of experts was the Jubilee Lecture of the Institution of Electrical Engineers. This Society was founded in 1871, under the name of the " Society of Telegraph Engineers and Elec-

tricians," but the name was changed in 1888 to the title above given.

The first ordinary meeting was held on February 28, 1872, and hence, in February 1922, it was resolved to have a Jubilee Celebration. I was invited by the Council to give an experimental lecture twice in the day, afternoon and evening. The subject I selected was "Faraday and the Foundations of Electrical Engineering," intended to expound the practical bearings of Faraday's great electrical discoveries.

I made a request of the Committee of the Royal Institution, through my friend Sir James Dewar, to be allowed to borrow for the day Faraday's original pieces of apparatus made with his own hands. This great favour was allowed, and these almost sacred relics were placed on my lecture-table.

I repeated before my audiences the great epoch-making experiments of Faraday, with apparatus identical with that used by him. The lecture was a great success as regards the experiments, and an electrical journal, *The Electrician*, referred to it as "A triumph of delivery and stage management." The afternoon audience included some of our most notable scientific men, and in the evening the theatre was crowded with students and practical engineers engaged in the day.

Among the very special lectures I have given to experts, the fourteenth Kelvin Lecture must be mentioned. This lectureship was established in 1907 by the Council of the Institution of Electrical Engineers in memory of Lord Kelvin, and had been given thirteen times when I was invited to give the fourteenth lecture on May 10, 1923. I selected as my subject, "Problems in Telephony, Solved and Unsolved."

The custom is to take some subject bearing on the life-work of Lord Kelvin. As he brought over and exhibited at Glasgow in 1876 one of the first Bell Telephones seen in this country, the subject I selected was appropriate. I had a large and very distinguished audience of telephone experts and eminent scientific men.

For some years past the Physical Society of London has been accustomed to hold an Exhibition of Physical Apparatus in January at the Imperial College of Science, London, to make known recent advances in Physics. It has been usual to invite some one to give a public lecture on some topic in general interest. I have twice been invited to undertake it, once in 1930 and again in 1934.

In 1930 I took as my topic the subject of "Television," and Mr. J. L. Baird, the chief pioneer in this subject, kindly made a demonstration of his apparatus for transmitting images

of living objects to a distance, and by numerous lantern slides I explained the *modus operandi* of it. The lecture was so successful that it had to be repeated a second time on the same evening, to accommodate all who wished to hear it.

On January 11, 1934, I was again asked to give the discourse on the "History and Development of the Thermionic Valve." I showed the principal experiments which led to the invention, and described the contributions of various inventors which have led to the modern valve in its present form. It has been described as the "master weapon of the Radio engineer." Without the thermionic valve wireless telegraphy would be very limited, and broadcasting and short-wave wireless would not exist at all. All the modern and extraordinary developments of radio work, and television as well, are absolutely dependent on (and would not exist but for) the thermionic valve as at present made.

Many of the courses of lectures above described have been subsequently written out and amplified by me into books published at various times. One of the first of these, called *Short Lectures to Electrical Artisans*, published in 1885 by Messrs. Spon & Company, was a set of lectures given at the request of Colonel R. E. B. Crompton, F.R.S., to the pupils and workmen

at his Electrical Works at Chelmsford. This was afterwards enlarged into a book called *Magnets and Electric Currents*. Three of the courses I have given at the Royal Institution, namely, an afternoon course on "Electric Lamps and Electric Lighting" and the two sets of Christmas Lectures, one on "Waves and Ripples" and the other on "Wireless Telephony," were published as books, and met with much appreciation.

Two of the courses of Advanced Lectures given to specialists for the University of London, one on "The Propagation of Electric Currents in Telephone and Telegraph Conductors," was given at University College in 1910, 1911, and 1916; and another, attended by nearly four hundred men, was given in 1926 at the Institution of Electrical Engineers, on "Scientific Research and Electrical Engineering." Of the two last courses the first was given to engineers in the General Post Office, to bring to them recent information on telephonic theory, and the second to make evident to practical electrical engineers how recent advances in practice depended essentially on pure scientific research.

Four of the other books published by me were on wireless work. My principal books are the two published by Messrs. Longmans, Green & Co., on *Electric Wave Telegraphy* and on

Elementary Manual of Radio-Telegraphy. The first of these occupied nine months in writing, and was first published in 1904, and has run through four editions. But books on this subject very rapidly get out of date, owing to the speedy advances in theory and practice.

In the same manner my book on *The Thermionic Valve*, though well up to date when first published in 1919, and fully revised in a second edition in 1924, has now fallen behind. Also since the War the book-buying power of the public has greatly decreased, and the modern wireless "fan" does not require elaborate theory but a cheap book to show him how to make his own wireless set.

The whole aspect of wireless work was, in fact, revolutionized by the discovery that the three-electrode thermionic valve could generate electric vibrations as well as detect them. The power to produce in this way electric oscillations in continuous or unbroken trains of waves in place of the short die-away trains generated by condenser discharges, made an absolutely new departure, and all books dealing chiefly with so-called spark wireless telegraphy have become back numbers.

The invention of the valve and its evolution is one of the most remarkable of electrical advances. Again, the production of the screened

grid valve and Pentode valve has made possible the use of receiving sets without external aerial wires, but with internal frame aerials which will yet pick up British and foreign radio-stations perfectly, and has created the vast new industry of broadcasting and wireless reception. In a small book called an *Introduction to Wireless Telegraphy and Telephone*, which I wrote for Messrs. Sir Isaac Pitman & Sons in 1924, in the final paragraph I wrote as follows concerning wireless telephony: "Wireless telephony is indeed a marvellous application of scientific discoveries, and has before it a vast field of usefulness. It is wonderful to think that at some future time, perhaps not far distant, a single human voice may be able to address listeners scattered over the whole surface of our globe."

This prophecy of mine was remarkably fulfilled eight years later, when, on Christmas Day, 1932, and also on the same day, 1933, our gracious King George V. was able to speak a Christmas greeting to his subjects over all his Empire, and be heard speaking clearly by wireless in Australia, New Zealand, Africa, Canada, and in Great Britain in his personal voice, making thereby a new and closer link between the Throne and the People of his world-wide Empire.

Another book on the *Alternating Current Transformer*, first published about 1888, was issued first as a series of articles in a journal called *The Electrician*, and ran through a number of subsequent editions.

Altogether, about twenty books on scientific subjects have my name on their title-page, including two which are not strictly text-books of Science.

CHAPTER VIII

TRAVELS AND RECREATIONS

ALTHOUGH the greater part of my daytime life has been spent in lecture-rooms, laboratories, and libraries, I have always had a very keen joy in natural scenery and in the beauties of Nature, in mountain, sea, lake, and forest. Also travel in foreign countries or to scenes of great historical events has been a special pleasure. My visits to Switzerland and to the chief Alpine centres, Zermatt, Chamounix, Wengen, and Pontresina in the Engadine, and Cormayeur, have been very frequent.

Although not an Alpinist in any very strict sense of the word, I have climbed a few of the high Alpine peaks, enough to become familiar at close quarters with the entrancing beauty of glaciers, ice slopes, and snow-clad summits.

In 1885, at Chamounix, I felt an urge to climb Mont Blanc. It is not as difficult as many lower mountains, but is fatiguing and rather dangerous in uncertain weather. With a competent guide

and porter, I left the Coutet Hotel one lovely afternoon in settled weather, and strolled up through pine forests and by mountain paths to the edge of the Glacier du Boissons, which must be crossed to reach the little hotel or hut, where one has to pass the first part of the night, called the Grands Mulets.

When just on the edge of the glacier we came across an American gentleman, alone, without guide or other companion, and we asked where he was going, and he replied that he had been thinking of crossing the glacier to get to the Grands Mulets on the other side. We assured him that if he attempted such a foolhardy feat as to try to cross a glacier alone, he would probably find himself, before many minutes had passed, dying a lingering and painful death at the bottom of a crevasse, or deep crack on the ice, beyond all human help.

Many of the accidents in the Alps result from reckless attempts to make expeditions without proper guides, or in uncertain weather, or without the essential requirements of ropes, ice axes, ample food, suitable dress, and especially well-nailed mountain boots. However, being so provided, I crossed the glacier in safety and reached the hut. There the guides lit a fire and prepared some food, and we lay down for an hour or two in simple beds.

Leaving the hut about 3 a.m., we went out to find our way by the light of a lantern to the edge of the great snowfield, which must be crossed before reaching the steep inclines which lead to the summit. It was very cold, the stars shone like diamonds on the deep blue sky, whilst in the east a faint gathering light spoke of the coming day. It is a long and fatiguing tramp up snow slopes, but it is necessary to reach the top, and get back off the deep snow, before the sun climbs high and softens the snow too much.

We reached the summit about 8 or 9 a.m. in a cloudless day. My pulse was then beating about 120 a minute. I dropped on a shawl my guide laid on the snow, and smoked a pipe for a few minutes, and swallowed a cup of very cold wine. Then I could stand up and enjoy the view. Switzerland lay like a map at our feet. All surrounding summits are dwarfed, and Mont Blanc stands supreme amidst her companion Aiguilles. I think on a clear day one sees more than a couple of hundred miles in every direction.

Then after a rest, the long tramp down began. We picked up some baggage at the hut, and, crossing the glacier again, reached the lower slopes. Finally, about 6 or 7 p.m., I dragged a very tired body into my hotel, and

after a hot bath and cup of bread and milk flung myself into bed for a dozen hours, pleased, however, to have climbed the highest mountain in Europe without mishap.

Two years later, in the company of an experienced friend, Mr. Howard Barratt, I climbed one of the five summits of Monte Rosa, ascending it from Zermatt. It is the second highest mountain in Switzerland. The weather was rather windy, and not so perfectly fine as on my Mont Blanc ascent, but we accomplished the task we had set ourselves with safety and success. In all Alpine ascents the weather is the uncertain factor, and it is never wise to attempt a high mountain except in perfect weather.

On one occasion with the above-mentioned friend we had a narrow escape. We had left a place, I think, Innertkirchen, to cross the head of the Rhone Glacier, spent the early night at a hut, and resumed our journey next morning. But it came on to snow, and after a little time the guides held a council of war. Should we press on and try to reach our destination, or return to the hut whilst we could yet see our footsteps in the snow? The latter decision prevailed, but we only just got back to the hut before the falling snow had obliterated all our tracks. If the snow had continued, we might have been shut up there without food, but

fortunately for us the snow soon ceased falling, and we were able to get back to the place from which we had started the day before.

In company with the same friend, I made a number of smaller ascents, and once crossed the Col du Géant to Cormayeur from Montenvert, in the valley of Chamounix.

Many times I have visited the Bernese Oberland, with its monarch mountains the lovely Jungfrau, attended by the Eiger, Monck, and Wetterhorn, which last I climbed with two other men and three guides in 1887. The Engadine district, with charming Pontresina as its chief centre, is very familiar ; and a winter visit to Wengen gave me a sight of these Oberland mountains in their winter dress.

Several of my foreign journeys have been in search of scientific knowledge in the early days of electric lighting. In 1884 the British Association met at Montreal in Canada, and I made it the occasion of a visit to the United States as already mentioned, to see the first Central Electric Supply Station put up by Edison in the lower part of New York City. After peeps at Boston and Philadelphia, and the scenery of Lake George and the Hudson, I made my way to the Falls of Niagara, as the project was then being first mooted for tapping the power of the Falls for electric current generation.

The Falls of Niagara, before they were mutilated for commercial purposes or by the growth round them of an industrial city, were one of the most impressive sights in the world. They are divided into two parts by Goat Island, the American and Canadian or Horseshoe Falls lying on opposite sides of the island. The space into which this vast volume of water is poured is a boiling cauldron covered with a mist of spray. When the sun is shining, and the spectator is in the right position with back to the sun and mist in front, countless rainbows fill the view. It is possible for persons to go a little way under the Falls on the extreme Canadian side. Walking down a steep path to the foot of the Falls on the Canadian side, and dressed in complete waterproof overalls, and firmly taking the arm of a guide, one can walk along some slippery planks a little way under the Falls. The torrent of water falls like an emerald green curtain over the spectator, an indescribably beautiful sight, but the noise, wind, and spray take one's breath away, and few people can stand it for more than a minute or two.

A great volume of water is now, however, deflected into lateral canals before reaching the Falls, and rushes down iron tubes called penstocks, driving turbines at the bottom which are coupled by long shafts with dynamo machines

at the higher level in large power-houses, for distribution of electric current to factories near.

The Horseshoe or Canadian Fall is half a mile wide and one hundred and fifty feet high. From it now a little under half a million horse-power are drawn off, whilst from the American Fall more than half a million are also taken. It is chiefly utilized in factories close by for the production of various chemical materials, but some of it is transmitted to a distance of as much as two hundred miles.

In the days when I saw the Falls in all their pristine beauty and grandeur, in 1884, this abstraction of power had not begun. I was, however, one of the electricians consulted as to the system of electric generation and distribution which should be adopted when the work commenced.

Travelling on from Niagara, I visited Toronto, attended the British Association Meetings at the fine city of Montreal, and voyaged down the river St. Lawrence to the French city, Quebec, magnificently situated on a high cliff above the river.

On other occasions I made journeys to Italy, as the utilization of water power there for electric supply was rapidly proceeding. A large hydro-electric station had been established at Tivoli, twenty-five miles from Rome, to employ the

great waterfall there for electric supply in Rome, and Professor Mengarini gave me all possible facilities for seeing it. I took the opportunity to continue my travels as far as Naples, to visit and ascend Vesuvius. Naples was then quite remarkable for the hordes of beggars who solicited alms from all well-to-do persons. I remember them well running for miles by the carriage in which we drove from Naples to Resina.

Then after lunch at an hotel, the cone of Vesuvius is ascended by a wire rope or funicular railway, followed by a short walk from the upper station. At this last place crowds of insistent guides lay hold of the traveller, and it used to be said that since the Italian Government had put down brigandage as a profession, all the brigands had become guides on Vesuvius. The only plan to avoid being torn in pieces was to select your man instantly, and promise him five lira for taking you to the top. When the wind is strongly in one direction, so as to blow the sulphurous vapours away, it is possible to walk to the windward side of the crater, and look down into the infernal regions.

At the time I was there, a small eruption had been taking place. A river of red-hot lava was flowing down one side of the mountain, and at all parts of the crater edge the ashes were hot

enough to cook hard, in a few minutes, eggs put into them by the guides, and, I may add, do damage to one's boots as well. Ashes and stones were being thrown up from the crater outlets, and by night the tall column of smoke ascending in the air was illuminated by the red glow of subterranean fires. A visit to Vesuvius under such conditions gives one a vivid idea of the enormous heat energy which is latent near the surface of our globe.

A visit to Pompeii, overwhelmed by dust and ashes ejected from the volcano on August 25, A.D. 78, served also to show the dangers which are ever present on the edge of an active or even latent volcano.

In the winter of 1928-29 I had the opportunity of making one of the longest and most interesting excursions of my life to Egypt, up the Nile, and to Palestine and Jerusalem. On December 7, 1928, I left London on a fine winter day for the journey to Marseilles, crossing the Channel in the turbine steamer *Biarritz*. We reached Paris at 4 p.m. I drove across to the Gare de Lyon and secured my seat in the night train. The permanent way in French railways is not sufficiently good for the heavy rolling stock, and in the case of long express night trains the carriages shake most unpleasantly. I had no sleep that night, and reached

Marseilles very tired. My passage to Alexandria was booked on the s.s. *Champollion*, one of the largest boats of the Messageries Maritimes Line, 15,000 tons and 10,000 horse-power. The ship has a length of 494 feet and breadth of 63 feet, and the cabin accommodation is luxurious. It is, in short, a great floating hotel. We voyaged the first day between Corsica and Sardinia, and ran three hundred and twenty-six miles in twenty-four hours. Later, we passed through the Straits of Messina, but the night was cloudy and I saw nothing of Etna. In four days we were only thirty miles off Alexandria.

The arrangements for disembarking were of the most unsatisfactory kind. The Custom House and railway station are some way from the dock. Crowds of natives, who seemed not to have been washed since they were born, tried to snatch one's baggage to earn a few piastres, and it was a matter of great difficulty to reach the station and secure a seat in the train to Cairo. A four hours' journey brought us, however, to the Capital City of Egypt, and to Shepherd's Hotel, of world-wide reputation. Every one who is anybody has sat on the piazza of this famous caravanserai. It is on the margin of a dusty street, and in some respects the Semiramis Hotel by the margin of the Nile is far better ; but Shepherd's Hotel is near the rail-

way station and to Thomas Cook's Excursion Office, to which most people have to go to change circular notes or get excursion tickets.

A day or two after arrival my dragoman took me to see the Pyramids of Gizeh and the Sphinx. One does not at once appreciate the immense mass of these historic buildings. Herodotus tells us that it took 100,000 men twenty years to build the Great Pyramid, and every one of the vast stones, three or four feet high and six feet long, of which it is mostly built, had to be brought from long distances. The white limestone casing stones have long been torn off, but when new the Pyramid must have been a wonderful sight.

The Sphinx also is enormous. It has the body of a lion and a human head, but the features have been broken long ago. Mounted on a donkey and my dragoman on a camel, we rode round two of the three Pyramids of the group, but I did not care to climb up one, as the only orthodox way to ascend is to be dragged up bodily by two dirty Arabs and pushed up behind by a third. The interior passages of the Great Pyramid are wonderful, but I was advised not to attempt an inspection.

Close by the Pyramids is a very good modern hotel, "The Mena House," where many people stay.

My readers will probably be aware that there are various theories or explanations of the purpose of this stupendous structure, the Great Pyramid. The Egyptologists are mostly agreed it was the tomb of a King Khufu, of the IVth Dynasty. Others, such as Mr. R. A. Proctor and Professor Piazzzi Smyth, have advocated the theory that it was an astronomical observatory, and that the entrance passage was directed in 3400 B.C. to the star *Theta Draconis*, then the Pole Star. Other speculations have been made that it was a divinely-inspired standard of weights and measures, and others still that it embodied predictions of world history. I will not for a moment pretend to decide between them.

Returning, then, to Cairo, I went on the following day to visit mosques and bazaars in old Cairo, and other sights such as the Mosque of Amr, one of the oldest, and the Mosque of Mahomet Ali, one of the most beautiful. In visiting these religious buildings one is supposed to do so with bare feet—"Put off thy shoes from off thy feet, for the place whercon thou standest is holy ground." But if a European were to leave his shoes or boots at the door he would not find them again, as they would be stolen in two minutes. Hence at the entrance to these holy places a lot of men sit with a collection of old yellow leather slippers. A pair of these

they tie on over the visitors' ordinary footwear, and he is then supposed to have bare feet. On coming out, he pays a piastre ($2\frac{1}{2}$ d.) or two for the accommodation.

The main street of Cairo is a dusty place traversed by donkeys, camels, motors, horse carriages, and electric trams; and hordes of natives press you to buy walking-sticks, bead necklaces, and lucifer matches at every step.

Quite near the Nile is the Museum of Antiquities, with its priceless treasures of exploration. I carried with me an introduction from Sir W. Flinders Petrie, the celebrated Egyptologist, to the curator, Dr. R. Engelbach, and he allowed me to see the mummies of the Great Pharaohs, Seti I., Rameses II., and Merenptah, which are not shown to the public.

It was at one time generally assumed that Merenptah, otherwise Merneptah, was the Pharaoh of the Exodus of the Israelites, but opinion now inclines to the view that the great warrior Thothmes III. was the Pharaoh of the Oppression, and Amenhotep II., or Amenophis II., was the Pharaoh under whom the Exodus of the Israelites took place. The title "Pharaoh" merely means "Great House," and it is a general title like the "Sublime Porte" or "Emperor." The Old Testament does not give us the personal names of the so-

called Pharaohs, except in one case, and hence a certain difficulty of fixing the chronology of events.

On another day I was taken by Mr. Mathias, the engineer-in-charge of the Marconi Beam Stations in Egypt, to see the Receiving Station at Maadi, about eight miles from Cairo, and there saw and heard the Beam wireless messages coming from England. At that time H.M. King George V. was very ill, but we used to receive the bulletins in Cairo a few minutes after they were issued in London, allowing for the difference in time.

After a fortnight in Cairo (far too long) I left for a visit to Jerusalem. Four hours by train from Cairo brought me to Kantara, on the edge of the Suez Canal. Crossing this by a ferry, we entrained by the night train for Jerusalem, an eight hours' journey over the desert. It was full moon, and by its faint light the sand glistened like snow. At 7.45 a.m. we reached Ludd (the ancient Lydda), and changed carriages for the two hours of the long ascent of two thousand feet up to Jerusalem. With all its religious traditions and associations it seems strange to arrive at a railway station, labelled "Jerusalem," similar to countless of others in England, and find porters waiting for the traveller's luggage and cabs to take him to his

hotel. I stayed at the Allenby Hotel, not far from the Jaffa Gate, by which gate Lord Allenby entered Jerusalem when he had driven out the Turks in 1917.

The first place to which guides conduct the visitor is to the Church of the Holy Sepulchre. It has been for fifteen hundred years the traditional site of the crucifixion and tomb of Christ, but there are strong arguments against it, and many authorities consider that a rounded hill a little outside the Damascus Gate, on the north side of Jerusalem, which is now occupied by a Mohammedan cemetery, has far better claims to be the true site of that stupendous event.

On the following day my dragoman took me up to the Temple Area to visit the Mosque of Omar, which has stood for nearly twelve hundred years in the position, on the summit of Mount Moriah, once occupied by the Temples of Solomon and Herod. It has a magnificent interior, and its stained-glass windows, marble pillars, carpets, lamps, and curtains are very beautiful. Under the dome and surrounded by a marble wall is a rough protruding rock. This is a place sacred alike to Jew, Mohammedan, and Christian. It is the summit of Mount Moriah. It is by tradition the place on which the Altar of Burnt-Offering stood in the Jewish Temples, and by a more uncertain tradition the

place where Abraham offered up Isaac, and where the vision of the destroying angel appeared to David, the King.

The same day I motored to Bethlehem and the Church of the Nativity, and on the following day to Jericho and the Dead Sea. The road to Jericho is a steep road, twenty miles long, down a most desolate valley, and it is even now still possible to "fall among thieves" on the journey, as did the man in the Gospel parable. The river Jordan flows into the Dead Sea, but there is no exit for the water except by evaporation. It lies about 1300 feet below the level of the Mediterranean Sea. The specific gravity of the Dead Sea water is considerably greater than ordinary sea water. Any one who attempts a swim in the Dead Sea water has great difficulty in keeping the legs under water, and emerges from his dip crusted with the salts which give it such unusual density.

Eight miles from the head of the Dead Sea, Jericho is situated. There are in reality three places called Jericho. There are first the ruins of the ancient Canaanite city, which are now being excavated by Professor Garstang for Sir Charles Marston. It has been found that the walls fell flat outwards, possibly by an earthquake, and that the city was undoubtedly destroyed by fire as related in the Book of

Joshua at a date close to 1447 B.C., which agrees with an exodus in the reign of Pharaoh Amenophis II. or Amenotep II., as already stated. There are, then, the remains of the Roman Jericho of the time of our Lord, and a modern town with a large hotel.

Visits to various other places in and near Jerusalem concluded a week or more of intensely interesting visits to sacred and historic sites, and I then returned to Cairo.

My travels that winter were extended by a voyage in one of Thomas Cook's excellent Nile steamers, the s.s. *Damietta*, up the Nile as far as Assouan and the First Cataract and Dam. It is an extremely pleasant experience. The cabin accommodation and dining-saloon service is excellent on these boats. The journey began with a train run of seven hours from Cairo to Asyut, where the ship waited for us, and we started on the up-river voyage whilst we were having afternoon tea on deck. By day we steamed slowly up-stream, and by night often laid up at some landing-place. There are in the Nile abundant shoals, and men at the steamer bows continually wield a pole to feel for them, as if the boat ran on a sandbank it might be hours before it could be got off again. At frequent intervals we were landed to visit some temple or historic spot—for instance, at Dendera

we were taken to see the great Temple of Hathor, built on the foundations of a much older temple.

In a few days we reached Luxor with its magnificent hotel and its great temple in ruins, the work chiefly of Amenhotep or Amenophis III., and added to by Rameses II. This last Pharaoh had an egregious vanity and a habit of erecting large statues of himself on very small provocation.

A still more magnificent creation of Egyptian builders was the great Temple of Karnak, near Luxor. The ruins of the immense hypostyle with one hundred and thirty-four gigantic pillars, some twelve feet in diameter and one hundred feet high, is one of the wonders of the world.

A still more exciting trip was that to the Valley of the Kings, about ten miles from Luxor on the west side of the Nile. Here in a desolate valley, with not a shred of grass or single tree, are the tombs of some of the great Pharaohs, and the greatest "find" among them being the tomb of Tutankhamen, discovered by Mr. Howard Carter and opened by him and Lord Carnarvon in November 1922. These tombs were excavated at the end of long passages driven into the side of the cliffs. The galleries were closed by many doors and pits to secure

their treasures from tomb robbers. At the end was the mortuary chamber, where the body of the embalmed king was laid in the interior of many coffins, and finally enclosed in a stone or alabaster sarcophagus. Vast treasures and ornaments were buried with the monarch, and in nearly all cases the tomb had been despoiled by ancient thieves. We visited, however, the tomb of Seti I., whose mummy is in the Cairo Museum, and of Rameses VI., and saw Amenophis II., who was probably the Exodus Pharaoh, lying in his stone coffin. These royal tombs are now lit with electric light, and are visited by hundreds of tourists each year.

The great show place at present is the tomb of Tutankhamen, and the body of the king has been left lying in his open coffin. Much of the treasures found in the tomb are now in the Cairo Museum, and I there saw the inner gold-plated coffin of the young Pharaoh. I was told the gold alone was worth £60,000.

Returning in the afternoon, we had lunch at a rest-house close to the Temple of Hatshepsut, that most wonderful woman who some suggest was the daughter of Pharaoh who took care of the infant Moses found among the bulrushes of the Nile. She was the daughter of Thothmes I., and sister of Thothmes III.

We steamed up as far as the Assouan Dam,

and had interesting excursions to Elephantine Island, the Cataract Hotel, and to the half-submerged Temple of Isis, on the island of Philae, above the Dam. This dam is about one and a quarter miles long, and has one hundred and eighty gates through which the water flows and is controlled. The purpose of this dam is to regulate the flow of water down the Nile. The only fertile places in Egypt are those on either side of the river, which can be irrigated by the overflow water of the Nile. If the Nile is too low or too high in flood, the result is failure of crops and hence famine. In former times famines took place at intervals. But now there is perfect control of the river, and the result has been an immense increase in the prosperity of Egypt.

One of the most memorable of the experiences of a voyage up the Nile is the arresting beauty of Egyptian sunsets. When the sun touches the western horizon, it leaves behind it when it sinks lines of scarlet or crimson flame set on a background of blue-green sky. The sunset colour is often reflected in the eastern sky more faintly, and as it slowly fades away and stars come out, it leaves on the mind an ineffable sense of rest and peace. On one occasion as we returned after a long day's excursion to various temples and ruins, closing with an ex-

ceptionably beautiful sunset reflected in the waters of the Nile, one of our party said, "The thing I shall remember best is this sunset."

Returning to Cairo, I left early one morning for Alexandria, and took passage on board the s.s. *Helouan* to Trieste, in the Adriatic, from which place a long journey across Italy and the south of France, in easy stages, brought me once more to my native land.

I ought not to omit to mention amongst my scientific memories the journeys to Switzerland and Norway to examine the recent electrical developments in those countries, to aid me in my professional work as an electrical engineer. The places are particularly rich in water-power available for utilization, whilst at the same time they have little or no coal deposits or oil springs. Thus they formerly had to import all the coal required for making steam either for stationary or locomotive engines.

When, however, the advances in electrical engineering had rendered it possible to drive dynamos or alternators by turbines driven by water falling from a great height, it became possible to use the natural water resources to generate electric current at high pressure, which could be transmitted long distances and utilized by electric motors for traction, lighting, and manufacturing processes.

Long before the Great War, in 1914-18, engineers in Switzerland had begun to consider the problem of working the Swiss railways electrically, and this was rendered absolutely essential when imports of coal from Germany were cut off in the War and steam coal rose in price to about £8 per ton.

In 1924 and 1925, having secured good introductory letters, I paid visits to Switzerland to examine the arrangements in detail for working the main transalpine railways electrically. There are two great electrical firms in Switzerland, one Messrs. Brown, Boveri & Co. (called generally the B.B.C.), whose works are at Baden, and the other called the Oerlikon Company, whose works are at Zurich.

Broadly speaking, the method of working is to construct in various places what are called hydro-electric stations as follows: Some high level lake is selected, or river coming down from a great height with abundant supplies of water all the year round. From this lake or river great steel pipes are laid down, which are often five or six feet in diameter, leading to some building at a much lower level. These tubes bring down the water at a great pressure to turn water-wheels of a certain kind enclosed in an iron case, called turbines. These turbines are connected to and drive electric machines called

alternators, in which coils of wire are moved rapidly in front of magnets, and thereby have electric currents created in them. These currents flow backwards and forwards in the wire, changing their direction two thousand times every minute. They are called alternating currents.

These currents can have their pressure or voltage increased by transformers, as explained in a previous chapter, and the energy is transmitted long distances on thin wires. Then the pressure can be reduced and the low voltage current used for driving an electric motor, which in turn may drive a train.

It is sometimes necessary to convert this alternating current into a direct current, or one flowing always in the same direction, as for driving tramcars in cities, because then a type of electric motor can be used which is simpler and lighter than an alternating current motor.

This is done by a machine worked out with great skill by Messrs. Brown, Boveri of Baden. It is called a mercury arc rectifier. It consists of a steel box in the bottom of which is a pool of mercury or quicksilver. In the top of the box are certain iron rods which are insulated from the walls of the box. The air is all pumped out from the box, and it becomes full of the vapour of mercury.

This vapour has the property that it allows electric current to flow only one way through it, so it acts like an electric valve. In fact, it does on a large scale what my thermionic valve does for small currents; this rectified current is used to drive direct current motors on electric tramcars, and is now largely employed in city tramway and state railway working in many countries.

I may briefly mention the electric working of the two great main line electric railways in Switzerland, the St. Gothard and the Simplon, which pass through long tunnels under the Alps. The two hydro-electric stations which supply the St. Gothard railway line, running from Lucerne to Chiasso on the Italian frontier, are at Amsteg on the Swiss side, and at Ambri-Piotta on the Italian side of the St. Gothard Tunnel. The Piotta hydro-electric station takes its water from Lake Ritom, at a height of 6000 feet above the sea and 2600 feet above the station. The great water tubes are 4000 feet long, and deliver water at a pressure of 1120 pounds per square inch to the turbines, of which there are four each of 12,000 horse-power. The alternators coupled to them deliver in all about 25,000 horse-power in the form of alternating current at a pressure of 15,000 volts. This is conveyed to an overhead line which runs all along the

railway, and it is reduced again to 600 volts for working the electric locomotives.

The result has been to make a great improvement in the speed and comfort of Swiss railway travel. In the days of steam locomotives it was necessary to close all the windows of the carriages and ventilators to keep out the smoke of the engine in passing through the main tunnel, ten miles long, and scores of numerous shorter tunnels on the line. Now one can travel in much less time with open windows all the time. The station at Amsteg is very similar to that at Ambri, and the two hydro-electric stations are connected by an underground cable which runs over the Alps, so that they can help each other. This cable carries electric current at a pressure of 60,000 volts.

The Simplon line, which runs from Brieg to Milan, through the Simplon Tunnel, fourteen miles long, is worked from a large hydro-electric station at Chatelard on the Salvan Pass, which leads from Vernayez in the Rhone Valley to Chamounix in France.

About one and a half million horse-power are now taken from water sources for working all the main and subsidiary railways in Switzerland electrically.

In nearly all the travels above described I have made records of happy hours either by

photography or some imperfect water-colour sketching. Even if one does not attain any particular excellence in this latter art, it is useful in impressing upon the memory some of the infinite beauties of Nature and its exquisite colour and forms.

As regards landscape photography, I may perhaps here make mention of a device which serves to prevent useless waste of plates. Nature presents itself to the eye as a patchwork of colours, owing to the colour-sensitive property of the retina of the human eye. This contrast of colour does not, however, affect in the same way the photographic plate. It is the actinic quality of the light sent out from each area which is then the determining factor.

If one looks at the snowclad Alps with a blue sky as background, the contrast is great in colour to the eye, but if a snapshot is taken with a camera of the scene there is little outline of the mountains on the plate when developed, because both snow and sky radiate about equally in actinic power.

I found the best way to proceed was to make a metal diaphragm behind the lens with a very small pinhole opening as a stop, and to use a very slow photographic plate. The exposure then in a bright day may be five or six seconds, and it is easy by experience to adjust a time-

exposure to give sharp contrast between snow and sky in the developed plate and plenty of detail in the image as well.

By this device I have often secured very good photographs of snow-capped mountains with blue sky behind, and had enlargements made from half or quarter-size plates. The only objection is that one must carry a camera-stand for time exposures, but the result is worth the trouble.

CHAPTER IX

SCIENTIFIC FRIENDS AND RECOGNITIONS

IN concluding these brief memories of a scientific life it may not be altogether inappropriate to mention some remembrances of colleagues and scientific friends with whom I have been closely associated, and to whose friendship and support I owe largely, I think, the recognitions and distinctions which have come to me for my scientific work. No man succeeds wholly in virtue of his own merits, and an important aid and art in life is the power to make and keep the friendship of elders and superiors.

The first important recognition I received for my scientific work was my election as a Fellow of the Royal Society of London in 1892. There are from one hundred to one hundred and fifty candidates each year for this great distinction, but only fifteen to seventeen can be elected per annum by the regulations, and the selection rests entirely with the members of Council.

The procedure is for two Fellows to propose and second a candidate, and secure other signatures to a certificate of his scientific work. This is suspended in the rooms of the Royal Society for inspection. In my case the Fellow who kindly undertook to collect the signatures was Mr. Shelford Bidwell. He was a physicist of eminence, and one of the original directors of the Edison Electric Light Company of London. He was subsequently President of the Physical Society, and a pioneer in the researches which have given us Television. He studied particularly the action of light on selenium, and the possibility of using it to transmit visible images of objects to a distance electrically. He secured nineteen or twenty signatures of Fellows in support of my candidature for the honour of F.R.S., and I was deeply gratified by being elected at the first annual suspension of my certificate in 1892.

In 1910 I had the honour of receiving from the Royal Society the Hughes Medals founded to commemorate David E. Hughes. He was the inventor of the microphone and many other important scientific instruments, and particularly of a printing telegraph which gave him fame and fortune. He was born in England in 1831, but taken to the United States when seven years old. He became Professor of Music in Bardstown College, Kentucky, U.S.A.

Morse's system of telegraphy, which originated in the United States, made use of an alphabet, each letter composed of a dot or a dash, or a combination of the two signs. Hughes had the happy idea of inventing a telegraphic instrument with keys like a piano, which could print down the message in Roman capital letters at the receiving end when corresponding keys in the transmitter were pressed. It was most successful, and he made a large fortune by it. At his death he left funds to award a gold and silver medal and premium each year for physical researches, especially in electricity and magnetism.

Hughes was a charming and unassuming man, and was much appreciated in scientific society. I knew him well, and had several very kind letters from him. He not only invented the microphone, but he actually made (about the year 1880) a metal filings coherer, and discovered that it could be affected by electric sparks at a distance. If he had not been discouraged by the opinions of some eminent men to whom he disclosed this fact, he might, indeed, have anticipated Marconi and Lodge in the invention of spark wireless telegraphy.

Hughes resided at that time in Great Portland Street, and he found that, walking up and down the street with a Bell Telephone receiver to his ear (the receiver being joined in series with

a single cell of a battery) and his metallic filings "coherer" (comprising a glass tube loosely filled with metallic filings), he could hear sounds due to electric waves sent out from an induction coil which he had set working in his apartments.

His laboratory comprised some very simple apparatus on a tea-tray; but with small resources he made important inventions, such as the sonometer, for testing the human ear for sensitivity, and the induction balance, which could detect the difference between a good and a bad half-crown. Also he made interesting discoveries in magnetism. Some time after his death his original apparatus was recovered by Mr. A. A. Campbell Swinton, and placed in the National Physical Laboratory, South Kensington, London, where it can now be seen.

On November 7, 1910, I had the gratification of receiving from Sir Joseph Larmor, one of the Secretaries of the Royal Society, a letter as follows :

"SIR,—I have the pleasure to inform you that the President and Council of the Royal Society have awarded to you the Hughes Medal for your Researches in Electricity and Electrical Measurements. It is hoped that you may be able to attend the Anniversary Meeting of the Society on November 30, at 4 p.m., to receive the medal in person.—I am, Sir, etc."

The medal was handed to me on St. Andrew's Day, 1910, by Sir Archibald Geikie, then President of the Royal Society, and I attended the annual dinner at the Hotel Metropole on the same evening.

In 1921 a still greater distinction came to me, quite unexpectedly, in the award of the Gold Albert Medal of the Royal Society of Arts, London. This medal was founded to commemorate the services to science and industry of Prince Albert, Consort of Queen Victoria, and has been awarded annually since the year 1864 for important inventions or conspicuous services to the nation. It has been donated to four crowned heads—in 1865 to the Emperor Napoleon III., in 1887 to Queen Victoria, in 1901 to King Edward VII., in 1913 to King George V.

It is awarded on the recommendation of the Council of the Royal Society of Arts, and handed to the recipient at present by H.R.H. the Duke of Connaught, who is the permanent President. It was a matter of great surprise to me to receive, on June 14, 1921, a letter from Mr. G. K. Menzies, the Secretary, as follows :

“DEAR PROFESSOR FLEMING,—I have the honour to inform you that the Council of the Royal Society of Arts, with the approval of their President, H.R.H. the Duke of

Connaught, have awarded you the Society's Albert Medal, in recognition of your many valuable contributions to Electrical Science and its applications, and specially of your original invention of the Thermionic Valve, now so largely employed in wireless telegraphy and for other purposes. The Medal was founded in 1862 in commemoration of the Prince Consort, who was for many years the President of the Society, and as you will see from the accompanying list has been awarded to a large number of men of the highest distinction. I trust it may be agreeable to yourself to have your name added to the list.—Yours, etc."

This very great distinction was presented to me by H.R.H. the Duke of Connaught in his own residence, Clarence House, on June 27, 1921, at 11.30 a.m., in the presence of the members of Council. The Duke recited some of the work for which it was awarded, and I endeavoured to make a suitable reply, assuring him and the Council that I was deeply sensible of the great honour thus bestowed upon me.

I know from private communications that I owed the honour a great deal more to the friendship of certain eminent men than to my own merits. Among these friends I must especially mention the late chairman of Council, Mr. A. A. Campbell Swinton, and also Sir James

Dewar, with whom I was associated in scientific work on the electric and magnetic properties of Matter at very low temperatures for several years, researches which we conducted together.

Another great and quite unlooked-for distinction was the award to me, in January 1928, of the Faraday Medal of the Institution of Electrical Engineers.

This medal was founded at the Jubilee of the Institution in 1921, and the first recipient, in 1922, was the eminent mathematician, Mr. Oliver Heaviside. I was the seventh recipient. This medal was presented to me by Sir Archibald Page, the President, on April 19, 1928, on the occasion when Sir Oliver Lodge gave the Nineteenth Kelvin Lecture before the Institution. Very kind speeches were made by the President and by Dr. Alexander Russell, in making the presentation. Owing to the fact that the Kelvin Lecture was to be delivered immediately after, there was a large assembly of members, and it was described by one of the electrical journals, *Electricity*, as a red-letter day for the Institution, as follows :

“The meeting of the Institution of Electrical Engineers on Thursday evening was one of the great occasions of the Institution. There was an extraordinarily large attendance, and members could be seen

sitting on the floor in the gangways, and standing packed together at the back of the lecture-theatre.

“That this was so is not surprising, in view of the fact that Sir Oliver Lodge was to deliver the Nineteenth Kelvin Lecture, and also that the Faraday Medal was to be presented to Professor J. A. Fleming for his research work on the Thermionic Valve.

“The gathering included the presidents of the Institutions of Civil and Mechanical Engineers and the representatives of other engineering and kindred institutions and associations.”

“VALUABLE PIONEER WORK.

“Speaking of Professor Fleming, Dr. Alexander Russell, Principal of Faraday House, said that for many years the Professor had been closely connected with the development of electric lighting, the telephone, the telegraph, and wireless telegraphy and telephony. In all these branches of applied science, Professor Fleming had done valuable pioneer work. More recently his research had led to his invention of the Thermionic Valve, which had revolutionized wireless telegraphy and telephony.

“Professor Fleming, continued Dr. Russell, had always been *persona grata* to the Institution of Electrical Engineers. His papers were milestones on the path of progress.

“Mr. Archibald Page, before presenting the medal to Professor Fleming, said that the Faraday Medal was awarded not more than once a year by the Institution for very notable scientific or industrial achievements in electrical engineering, or conspicuous services rendered to the advancement of electrical science, without restriction as regards nationality, country, or membership of the Institution. It was an appreciation of world-wide services rendered to science and engineering.

“It would not be out of place, continued Mr. Page, for him to recall that it was only 100 years ago since Faraday made the discovery upon which modern heavy electrical engineering was founded. At a much more recent date Professor Fleming, in his turn, made the discovery which led to those great developments in weak-current electrical engineering, which had been such a notable feature of the past few years.

“Within the past few years facilities for international communication had been greatly extended. We could now telephone to most European countries as well as to the United States and Canada. A week or two ago it was possible to hold in that room half of the joint meeting of their Electrical Engineers.

“In both wire and wireless telegraphy equally notable advances had been made for the same reason. This increased ease of inter-communication could not fail

to have a definite effect on international politics, industry, and social intercourse.

"They were there that evening to honour the man whose work had made these things possible. They would all agree it was Professor Fleming's invention of the Thermionic Valve which had resulted in these advancements being made. The Thermionic Valve was indeed a revolution in physics.

"Professor Fleming received the medal from the President amid tremendous applause. . . ."

Another journal, *Electrical Industries*, made similarly kind comments on this occasion :

"PATIENCE HONOURED.

"The award of the Faraday Medal to Professor J. A. Fleming, F.R.S., by the Institution of Electrical Engineers, forms a fitting acknowledgment of hard, patient and painstaking work, not only in the field of research, but also in the training of the young engineer. For over half a century Dr. Fleming laboured in both fields of endeavour, and while his name will go down in scientific history as the inventor of the first Thermionic Valve, the work he did in training the young, and as the occupant of the chair of Electrical Engineering at University College, London, was of quite as much value to the industry. One of the stalwarts of the early days when electrical

engineering was probably better described as electrical pioneering, he has contributed materially to the development of the industry and its personnel, and set an example and a standard for those who follow after him that will be by no means easy to emulate."

I replied as follows :

Mr. President and Gentlemen,

It is not easy for me to express in a few sentences, and in words that seem at all sufficient, my *deep* sense of the honour you have paid me in this award of the Faraday Medal. When the news of it first reached me, I confess it gave me a great surprise, because I knew you were not limited in your choice to a member of this Institution or even to a British subject, but had all the world open to you for your selection. It is, therefore, all the greater distinction, and I can only say I am most keenly sensible of it, and most grateful for it as well as for the kind words of the President with which its donation has been made.

I may, however, say it is a peculiarly gratifying event, if only because much of such little research work as I have been able to do has been carried out at the Royal Institution, in the rooms and some of it on the very spot where Faraday made many of his great discoveries. For many years I associated myself with my

friend, the late Sir James Dewar, in work on the electric and magnetic properties of Matter at Low Temperatures, and we published some fifteen or twenty papers written by me on those results. Much of that work was done in the small basement back room in which Faraday carried out his magnetic researches, and where, I believe, he discovered the magnetic rotation of the plane of polarized light.

In this connection I should like to recall to your recollection that in about three years' time, namely, in October 1931, we shall reach the centenary of the date of Faraday's discovery of induced currents and magneto-electric induction, on which all the work of this Institution is built up.

Would it not be an appropriate thing for this Institution, perhaps in conjunction with the Royal Institution, to hold some celebrations commemorative of these great discoveries and their great discoverer? I will not say more now, because you are waiting to hear the Kelvin Lecture, by Sir Oliver Lodge; but I will conclude by thanking you once more, most emphatically and most sincerely, for the great honour you have conferred upon me in placing my name on the small list of those who are entitled to call themselves your Faraday Medallists.

In March 1928 I had the honour of receiving

from the Vice-Chancellor of the University of Liverpool an intimation that, at a forthcoming celebration of the twenty-fifth anniversary of the granting of a charter, the University desired to confer on me the Honorary Degree of Doctor of Engineering (D.Eng.). Needless to say, I accepted gratefully. The letter from the Vice-Chancellor was as follows :

From the Vice-Chancellor, the University of Liverpool.

March 7, 1928.

DEAR PROFESSOR FLEMING,—I am directed by the Senate and Council of the University to express to you their desire to offer you the honorary degree of Doctor of Engineering. On May 10 and 11 of this year, the University will celebrate the twenty-fifth anniversary of the granting of its Charter. It is our hope that if this invitation is agreeable to you, you will be the guest of the University on these two days, and join us in some or all of the short series of ceremonies which will then take place, receiving the degree at a special congregation on the afternoon of Friday, May 11.

Will you be kind enough at your convenience to let me know whether you will do the University the honour of accepting this invitation, and whether you can be present at our celebrations?—Yours very truly.

(Signed) H. J. W. HETHERINGTON.

To Professor J. A. Fleming, D.Sc., F.R.S.

It need scarcely be said I accepted the invitation with great satisfaction and pleasure. I was invited to stay during the days of the ceremonies with a friend, Professor E. W. Marchant, D.Sc., sometime President of the Institution of Electrical Engineers, London, and Professor of Electrical Engineering in the University of Liverpool. It was a very pleasant visit.

The Chancellor of the University was at that time the Rt. Hon. the Earl of Derby, and the degrees were bestowed by him in St. George's Hall, Liverpool, on May 11, 1928, in the presence of a large audience. There were twelve recipients of degrees, namely, Professor J. E. Littlewood ; Vice-Chancellor W. M. Childs ; Sir Archibald Salvidge ; Dr. H. A. L. Fisher ; Sir Edwin L. Lutyens ; Professor R. Robinson ; Professor Oliver Elton ; The Earl of Crawford and Balcarres ; Professor J. W. Gregory ; Professor T. P. Nunn ; Miss E. G. Holt ; and myself. Each recipient was presented by the Public Orator with description of the work for which he was honoured ; and after the hood and certificate had been bestowed, the undergraduates sang for each candidate some appropriate verses they had composed to a popular tune.

In presenting me, the Orator said :

“ My Lord and Chancellor,

“ John Ambrose Fleming has a conspicuous and most honourable place among those men of Science whose work has enriched and enlarged the range of human communication, and set at the service of men natural forces now constantly employed for their pleasure and their work, but until recently either unimagined or left in the region of the unreal and miraculous. He has had a great part in introducing to this country the Telephone, the Incandescent Electric Lamp, and Wireless Telegraphy and Telephony. He is the inventor of many cunning instruments, one of which, the Thermionic Valve, has given us Broadcasting and the Beam System of Wireless Telegraphy. He has multiplied and developed the discoveries of electrical science ; it may truly be said of him that he has spread light in the world. If, then, the dwellers in our towns and cities and in remote villages and hamlets owe him a great debt, we who live in a University owe him our thanks for fostering in the laboratory those studies, the utility of which he had proved to the large world. We do not forget that for forty-two years he was Professor of Electrical Engineering in University College, London ; a member of many learned and scientific societies, we desire to bring him within our own borders and ourselves to take a share in the honour which he confers on all those

who pay him the tribute of their respect and admiration.

“ In the name of the Senate and of the Council, I present to you, John Ambrose Fleming, D.Sc., F.R.S., for the degree of Doctor of Engineering, *honoris causa*, of this University.”

The community song as regards myself was :

Tune : “ *Comin’ thro’ the Rye.*”

“ When you hear loud-speakers speaking,
 Thank the Fleming Valve ;
 And the oscillator’s squeaking,
 Thank the Fleming Valve.
 Local news and cotton futures,
 Blame them all on me,
 But please remember that I didn’t
 Found the B.B.C.

Cockney humour, aunts and uncles,
 And the lovely talks,
 The foundation-stones of music,
 Syncopated squawks ;
 For our weary souls they form a
 Soothing, healing salve,
 We owe them all to this reformer
 And his Fleming Valve.”

This was sung with all the fervour of undergraduate verve and lungs.

I have already mentioned the foundation of the Physical Society of London by the late Professor Frederick Guthrie, and the favours I received from him. Guthrie was an experi-

mental physicist and trained as a chemist. He was a very charming and lovable man, endeared to all who knew him by his modesty and readiness to help younger men. He made several very important pioneer discoveries in physics. Perhaps the most important of these was that a red-hot iron ball on an insulating stand could retain a charge of negative electricity, but could not retain a charge of positive electricity. If it was white hot it could not retain, but would instantly lose, a charge of either kind of electricity imparted to it. This was an initial discovery in a now very important branch of electrical science called "thermionics."

As already mentioned, I had the privilege of reading the first paper before the Physical Society at its inaugural meeting in March 1874, and in succeeding years I read in all some thirty-six papers to that Society on various instruments I had devised or researches undertaken.

In March 1931, that is fifty-seven years after its foundation, the Physical Society presented me with the Duddell Medal, which had been founded to commemorate the work of Mr. Duddell, an eminent physicist. On December 8, 1930, I had the pleasure of receiving the notification of this award from Dr. Ezer

Griffiths, the business Secretary, in the following terms :

“ I beg to inform you that the Council of the Physical Society, at their meeting on December 5, 1930, awarded you the 1930 Duddell Medal for your work in the design of scientific instruments.”

This bronze medal is awarded not more frequently than once a year to persons who have contributed to the advancement of knowledge by the invention or design of scientific instruments or the discovery of materials used in their construction. The award to me was the eighth medal under the above conditions. The President at that date was Sir Arthur Eddington, the eminent astronomer and mathematical physicist, and his world-wide reputation as a scientific investigator of the first rank gave an added weight to his extremely kind words in presenting the medal—words which I am permitted to quote from the *Proceedings of the Physical Society* for September 1931 :

“ At the Annual General Meeting of the Physical Society, held at the Imperial College of Science, South Kensington, on Friday, March 20, the President of the Society, Sir Arthur Eddington, F.R.S., presented the Duddell Medal of the Society to Sir John Ambrose Fleming.

“ Sir Arthur Eddington, in making the presentation, spoke as follows : A careful review of the contributions to science and technology which have led to this award has been drawn up by those best qualified to judge, and I shall base what I have to say on their memorandum. We cannot sum up in a sentence researches extending over an active career of fifty years, but in general we may say that the work of Sir Ambrose Fleming has lain in the applications of electrical science, and perhaps particularly in the invention of instruments for exact measurement.

“ Educated at University College, London, the Royal College of Chemistry, and St. John’s College, Cambridge, he held for some time a lectureship at Cambridge in applied mechanics. From 1877 to 1879 he was working in the Cavendish Laboratory on a comparison of the British Association resistance coils and the standard British Association units—a work undertaken at Clerk Maxwell’s suggestion. For this purpose he designed in 1879 a special form of resistance balance, which was in use there for many years for comparing standard coils. He also designed a special form of standard coil capable of taking up quickly the temperature of the water in which it was placed.

“ In 1882, when practical incandescent electric lighting began, his attention was turned to the need for quick and accurate

workshop methods of electrical measurement. He made in 1883 the first direct-reading potentiometer, set to read current and potential directly in amperes and volts. He described this method in a journal which is now defunct. The method was almost immediately copied in various forms by all electrical instrument makers.

“In connection with incandescent lamp manufacture by the Edison & Swan Electric Lighting Company, Sir Ambrose Fleming designed in 1895 a form of large bulb incandescent lamp as a standard of light more constant and easy to use than the flame. This has been much used since. When alternating-current working began in 1883, and the alternating-current transformer began to be used, the necessity arose for careful determination of efficiencies. He designed a form of wattmeter for this purpose, with which he made extensive researches on transformer-efficiencies; these were reported to the Institution of Electrical Engineers in 1892. In this paper he first suggested the use of the term ‘power factor’ which at once came into everyday use.

“When practical wireless telegraphy first began under Marconi in 1898 no appliances were obtainable for measuring wave-lengths and frequencies. In 1904 Sir Ambrose Fleming invented a simple instrument for this purpose, which was capable also of measuring small capacities and inductances.

In conjunction with Professor Clinton he also devised a rotating commutator for measuring the capacities used in wireless. In 1904 he invented his now famous 2-electrode thermionic valve, which provided at once a simple detector of wireless waves, far more easy to manage than the coherer then in use. It prepared the way for a subsequent improvement, the 3-electrode valve, which is now the most important weapon of the radio engineer, and without which wireless as we now have it would not exist.

“In 1905 he described to the Physical Society an instrument called the campograph, which enables curves such as the hysteresis curve of iron to be delineated optically without the tedious procedure involved in other methods.

“Between 1892 and 1895 he co-operated with Sir James Dewar in a remarkable series of researches on the electric and magnetic properties of matter at low temperatures, including the great increase of electric conductivity which accrues. He devised special forms of resistance-coil and instruments for the measurement of inductance, capacity, and thermo-electromotive force. I may remind you of the immense advance in modern theoretical physics to which the study of low temperatures has led.

“He was Professor of Electrical Engineering at University College, London, for forty-two years, and he established there

the first laboratory for high-frequency and radio measurements. Besides his papers in scientific journals, he has published many books and treatises on applied electricity. It requires no great imagination to appreciate the importance of the numerous instruments of measurement designed by Sir Ambrose Fleming for use in the electrical industry generally and for scientific research, but I think there is one invention which comes uppermost in all our minds. I understand that the psychologists or psychoanalysts for their mysterious purposes sometimes fire at their victim a word or name which he has to respond to instantly by naming the first associated idea which comes into his head. If in this way I pronounce the word 'Fleming,' the instantaneous response is 'valve,' or we may vary the procedure and put it the other way round, and say when the word 'valve' is mentioned, 'Oh, Fleming, of course.' The discovery of the 2-electrode valve in 1904 has paved the way for the great development of wireless to-day, and so has led to one of the greatest influences of science on our civilization. This gift of science we can contemplate without those mixed feelings with which, for example, I should regard the telephone or the motor-car, which sometimes occasion our gratitude and sometimes occasion our profanity. I do not suppose that Sir Ambrose foresaw all the strange results that would ultimately

come from these experiments carried out in University College twenty-five years ago. As we sit here unheard voices are all around us ; from all parts of the earth they speak. As in the magic isle of Caliban,

‘ . . . the isle is full of noises,
Sounds and sweet airs that give delight and hurt not,
Sometimes a thousand twangling instruments
Will hum about mine ears : and sometimes voices . . . ’

“ I think it might be safest to end the quotation there, because Shakespeare is sometimes a little too apt. But if those in this audience who from time to time give talks before the microphone will shut their ears, I will complete the quotation :

‘ . . . that, if I then had waked after long sleep,
Will make me sleep again.’

“ It is a proud distinction to have been one of those responsible for an essential part of this device, to have overcome one of the major obstacles to the fruition of those ideas which started with Maxwell and Hertz. There are names which we honour in connection with the pure theory. We honour in a different way, though no less sincerely, those who by breadth of vision developed a scientific toy into something of world-wide service. Intermediately we honour those who by their skill in turning the results of scientific discovery into practical invention bridged the gulf, and among those the name of our medallist stands out conspicuously.

“ Now I turn to a feature of the award to which I have up to now deliberately suppressed all reference. I have, so to speak, placed our medallist on the dissecting-table, and tried to show you from his writings and inventions what manner of man he is. But Sir Ambrose Fleming is not altogether a stranger to the Physical Society. He is one of ourselves. In fact, if I may so put it, he is one of ourselves more than any of us.

“ To go back to a period when the age of the present speaker belonged to the domain of negative numbers, Fleming was one of the original members of this society, and in March 1874 he read the first paper at the first meeting of the society, his subject being, ‘ The New Contact Theory of the Galvanic Cell.’ That was a good start, and he kept it up. During the ensuing forty years he contributed regularly to our *Proceedings*—thirty-five papers in all, amounting, I suppose, to about one-third of the total of his published papers. Once a year the occasion of the award of the Duddell Medal gives the society the pleasure of recognizing and honouring distinguished merit. It is a double pleasure when the honour falls to one of our intimate colleagues in the society. It is a triple pleasure, a manifold pleasure, when it falls, as it does on this occasion, to a founder of the society, whose papers started our scientific proceedings, and who has been loyal to the

society during the fifty-seven years of its existence.

“Turning to Sir Ambrose Fleming, Sir Arthur Eddington said : Sir Ambrose, your life-work survives and bears fruit in the great electrical industries of the world. Your monument is in the circumambient ether. But you will not despise this mark of recognition and appreciation from the society of which you have so long been a member. In handing you the Duddell Medal I would express to you on behalf of the Physical Society its esteem for your achievements as a man of science, its regard and affection for you as a colleague, and its good wishes for your continued health and enjoyment.”

I made a speech in reply, in which some account was given of the origin of my principal scientific work, and I tried to express to the President, Council, and Members of the Society my deep sense of the honour thus bestowed upon me.

My old friend, Dr. H. E. Armstrong, F.R.S., as one of the original members, was requested also to speak by the President. He said :

“I have known my friend Fleming since he was a student in this college with Guthrie. He set an example from the very beginning, and I think all of us who have known him have loved him sincerely. Among

scientific men he has always ranked high on account of his extraordinary devotion to his work and his honesty in putting it forward.

“Sir Ambrose Fleming and I can overlook practically everything that has taken place in the development of electricity. As he has told you, the dynamo at the beginning was a very miserable machine, until Hopkinson took it in hand, and it had not been turned round and made into the efficient motor machine that we know to-day.

“Not only have we witnessed in the period of which Sir Ambrose Fleming has been speaking the whole development of applied electricity, but we have seen scientific development in many other fields. For example, we have witnessed the age of the ironclad. The first ironclad, the *Warrior*, was built in 1860; I saw her in 1865 at Gibraltar, alongside the *Marlborough*, which was the last of the wooden three-deckers. I saw the *Great Eastern* when she declined to be launched on the stocks at Greenwich, and, as you know, she laid the first efficient cable. Then there has been the development of the internal combustion engine. I have an acute recollection of our troubles at the City and Guilds of London Technical College at Finsbury in the early days; how, for example, the piston had a habit of falling down backwards. I recollect, too, a most

terrible quarrel between Ayrton and someone at Birmingham—Smith or Brown, I think, was the name—over this machine. It was a truly awful machine.

“One may say that one has seen a whole vast development of scientific knowledge and its application to machinery in general all in the space of about sixty years, and I think that is a matter that we ought to reflect upon a little more than we do. We do not sufficiently realize the astounding rate at which man has gone forward since he has learned to use the experimental method—the method in which your medallist has shown himself to be such a master. The world is in no way alive to the fact that we have discovered a new method, because it is practised only by the few, and nearly all of them engineers. We have been so much concerned with improvements in machinery, that we have forgotten the most important machine of all, that is to say, ourselves. We have not touched the politician, we have not touched government in any shape or form. I should like to see carried out a suggestion which was made by a bishop a few years ago, and put these interfering scientific inventors to sleep for a time, while we have leisure to think of other things. It seems to me that unless we can do that we shall be overcome by our own creations. Although we have to congratulate ourselves on this astounding development of the experimental method

as applied particularly in physics, we have to bear in mind that there is this very much more important work awaiting our direction."

Here also should be mentioned the Centenary Celebrations of University College, London, in June 1927, to bring to mind the foundation of that College in 1827, and its service to the country as a place of learning and education without religious tests, as was the case at that time with Oxford and Cambridge Universities.

His Majesty King George V. and Queen Mary graciously consented to open the Great Hall to commemorate those members of the College who fell in the Great War, 1914-18. The day selected was June 23, 1927, and was brilliantly fine. Everything went off perfectly. In company with a number of members of the College, including six of the oldest Professors, I had the honour of presentation to Their Majesties at the Assembly on the College lawn after the opening ceremony in the Great Hall.

A little before that day, on June 14, I gave, by special request, one of the Centenary Lectures at the College. My subject was "A Hundred Years of Electrical Engineering," and the chair at the lecture was taken by one of my old students, Mr. T. Roger Smith. I recounted briefly the story of electrical discovery during the years the College had been in existence.

One interesting recognition of my scientific work remains to be mentioned, and that is the honour of Knighthood bestowed upon me in March 1929, after my return from my visit to Palestine and Egypt.

On February 16, 1929, I had the pleasure of receiving an official letter from the Secretary of the then Prime Minister, Mr. Stanley Baldwin, stating that in the forthcoming list of New Year Honours Mr. Baldwin proposed to include my name, with the recommendation that the Honour of Knighthood be conferred upon me by His Majesty the King.

At that time His Majesty King George V. had been (and was) very ill, and unable himself to hold the Investiture, but by His Royal Command H.R.H. the Prince of Wales was deputed to give the accolade to those gentlemen selected for the distinction. Accordingly, on March 6, 1929, I received an invitation from the Home Secretary to attend at St. James's Palace, on March 27, at 10.30 a.m., morning dress being worn.

The ceremony was as follows : On arrival, the notified recipients of Honours were shown up into a large reception room, opening into the Throne-Room by two doors on opposite sides. After a wait, a gentleman described to us the procedure, and those to be knighted were

arranged in line in alphabetic order by the initials of their surnames. Then one by one we entered the Throne-Room by one door. The Prince was standing sword in hand under the canopy of the Throne with a cushion in front of him, and the Home Secretary, then Sir W. Joynson-Hicks, Bart., M.P., standing a few yards away. We were told to advance to the side of the Home Secretary, and wait whilst he pronounced our names and said a few words as to the reason for bestowing the Knighthood. Then we had to bow deeply, advance to the cushion, kneel down whilst the Prince gave the accolade or touch with the sword on each shoulder, and said, "Rise, Sir So-and-So." After that to rise and bow deeply once more, retire a few steps backward, bow again, and then leave the Throne-Room by the other door. All this was duly performed without a mistake, and I left the Presence a Knight Bachelor and a member of the oldest order of chivalry in Europe.

Later on, I had to attend at the College of Arms in Queen Victoria Street, London, by request of the Garter King-at-Arms, to sign the Garter's Roll of Knights Bachelor. Later on, I joined the Imperial Society of Knights Bachelor, and received the Insignia. I may add that I received a very large number of extremely kind congratulations from scientific friends and

organizations, with and for whom I had worked. Many of these were from men of great distinction.

The Master of my College at Cambridge, St. John's, the Council of University College, London, and very many others sent letters or telegrams, which gave me extreme pleasure ; and indeed it is this sympathy from friends, relatives, and co-workers that alone gives any real satisfaction in these matters.

Perhaps a few words as to the origin of my family, which goes back a long way, may be given here. I never had much interest personally in genealogical inquiries, but my third brother, Laremont, now deceased, investigated at one time very carefully our ancestry. The Flemings came, of course, originally from Flanders, and some of them settled in Scotland as early as A.D. 1020. A certain Baldwin Flandrensis was Sheriff of Lanarkshire in A.D. 1147. The surname then became changed to Fleming, and a Sir Malcolm Fleming was Sheriff of Dumbarton in A.D. 1246. The whole history of the descendants is given in a book called *Biggar and the House of Fleming*, published in 1867, by William Paterson, of Edinburgh. In 1321 the Flemings were supporters of Robert Bruce, and one of them was created Earl of Wigton. In the twelfth century, according to

the above book, the Flemings were perhaps the most active and enterprising people in Europe. As their native country, Flanders, soon became too small to hold them all, they emigrated in considerable numbers to England during the reigns of William Rufus and Henry I. Numbers of them also entered Scotland, and took service under David I., then on the Scottish Throne. One of these obtained a grant of land at Biggar, near Peebles, from that monarch, and became the founder of a family which, for seven centuries, held power in various ways there, and served in various capacities the Scottish Kings. One, Sir Robert Fleming, was created a peer by James II. of Scotland, and his successor, John Fleming, was Ambassador to France. But the family became impoverished by efforts on behalf of the beautiful but unfortunate Mary, Queen of Scots. It will be remembered that one of her four Maids of Honour was a Mary Fleming, a daughter of Lord Fleming. In loyally supporting that ill-fated queen, the Flemings, so to speak, "put their money on the wrong horse." They espoused also the cause of the Covenanters against the determination of the Stuart Kings of England to establish Episcopacy in Scotland, and this again reduced their prosperity.

To make a long story short, the title became extinct in 1747, by reason of failure of male

heirs. My father, the Rev. James Fleming, D.D., was descended from a younger branch. He was born at Kelso in 1816, and died in London in 1879. He was a man of great consistency ; no one ever detected in him the least undignified or unkind act, and he was greatly beloved by all those to whom he ministered in spiritual matters.

There comes a time, however, in the longest life when the physical body is no longer able to meet the demands on its strength, and moreover when it is wise to step on one side for the sake of younger men. This stage was reached by the author of this book in March 1926, when after forty-two years' tenure of the Chair of Electrical Engineering in University College, London, it was clear that it would be to the advantage of the College and University that the teaching of that subject should pass into younger hands.

Accordingly, I addressed a letter of resignation to the Provost of University College, Sir Gregory Foster, asking him to present it to the College Committee, and through them to the Senate of the University of London. My resignation was accepted by the Senate on May 21, 1926, and the Academic Registrar, Dr. Edwin Deller, sent me in a letter their resolution as follows :

“ That the Senate record their great regret at the resignation of Professor J. A.

Fleming, F.R.S., of the Chair of Electrical Engineering, held by him since its foundation in 1885 ; that they place on record their sense of the great importance of his achievements in this field of Applied Science, especially in connection with the development of Transformers, the Carbon Filament Lamp, the measurement of Electro-magnetic Waves, and later the invention of the Thermionic Valve ; and they express the hope that he may have many years of leisure and health to continue his scientific work in a field in which he has achieved such great distinction.

In the following July the Senate conferred on me the title of Emeritus Professor of Electrical Engineering in the University of London. Exceedingly kind letters of regret were also received by me from the Provost of the College, the late Sir Gregory Foster, from whom I had received much kindness, and from the Professorial Board and College Committee, and also from my colleagues in the Faculty of Engineering, as well as generous notices in many scientific and general periodicals.

During my tenure of the chair about two thousand students passed under my hands, and, as already mentioned, many of these have since attained high positions and great distinction in the electrical world. It was a matter of the greatest

satisfaction to me that when a decision was reached as to my successor in the chair, the choice of the Senate fell upon my Assistant Professor and old colleague, Mr. W. C. Clinton, B.Sc. He had been associated with me for about thirty-three years in the work of building up our Department of the College, and had rendered most valuable service by his zeal, ability, and efficient teaching. No better selection of an occupant of the chair could possibly have been made, and every one in the College felt the greatest satisfaction in his appointment as my successor in the chair. I felt that I had received almost more than I deserved when my colleagues and scientific friends made provision for the painting of a portrait of me by the celebrated artist, Sir William Orpen, R.A. The chairman of this Portrait Committee was my old friend, Mr. Alan A. Campbell Swinton, and a good deal of the work of organization was undertaken by Professor Clinton, who placed me in his debt by handing me an album with all the letters in it from contributors to the fund. The portrait was exhibited in the Royal Academy Exhibition at Burlington House in 1927 (No. 723). A replica of it was given to me, and by me was donated to the Institution of Electrical Engineers.

In 1932 a bronze bust of me was executed by

the well-known sculptor, Mr. G. H. Paulin, A.R.S.A., and exhibited in the Royal Academy Exhibition for 1932.

When I retired from University work and teaching, I settled in a small house I had built in the pleasant little Devonshire town of Sidmouth. My garden overlooks that of "The Glen," in which Queen Victoria as a little girl resided, and where her father, the Duke of Kent, died.

I have not altogether "burnt my boats," but still retain some active interest in scientific pursuits and progress. As President of the Television Society and also of the Victoria Institute, the latter formed sixty years ago to discuss questions lying on the borderland of Philosophy and Science and Religion, I am kept in touch with the "things that matter," and with the state and progress of scientific thought.

At the same time, my marriage to a lady, Miss Olive Franks, whose beautiful soprano voice and musical abilities cause her to be enthusiastically received and highly appreciated as a soloist whenever she takes part in any musical performance, added to her charms of disposition, has given me a companion who is always sympathetic and delightful, and moreover not only has encouraged me to write these memoirs, but greatly assisted me in revising the proofs.

I have had the good fortune to live through a most remarkable period of scientific history, in which astonishing advances have been made in practice and theory, and especially in the utilization of the electric and magnetic properties of matter.

In looking back over that period of time, the last half of the nineteenth century and the first third of the twentieth, over the greater part of which my memories of scientific work extend, one thing is very noteworthy in comparing the present with the past, and that is the enormous increase in our practical technical achievement, and on the other hand the diminished confidence we now have in the validity of our theoretical explanations of natural phenomena. There is, in short, at present far less "cocksureness" in scientific thought that we have plumbed the depths of knowledge or reached the end of possibilities in theory or practice. In the Charter of the Institution of Civil Engineers of London, the aim of Engineering is defined as that of controlling the great energies of Nature for the use and benefit of mankind. Nevertheless, it has been well said that Nature is a coy mistress, and will not be wooed simply for the sake of her dowry of applications. Scientific research must therefore go forward independently of any possible practical applications, and

it is certain that when thus pursued in a disinterested and reverent spirit it is one of the very highest occupations of the human mind.

The immense advances in astronomy in the last fifty or sixty years have given us an astonishingly enlarged view of the physical universe, and the corresponding theoretical advances due to the Relativity Theory of Einstein and the Quantum Theory of Planck, have created a revolution in our ideas as to invisible things in the world and behaviour of atoms.

There also seems to be a growing sense that the phenomena of the amazing Cosmos in which we live cannot be wholly accounted for by mechanical explanations or by an automatic impersonal evolution, but is in some inscrutable way the manifestation and outcome of a Supreme Intelligence. We may not be able to do more, in Newton's words, than gather here and there a shell or pebble on the margin of the great ocean of truth lying before us, but we have every confidence that persevering efforts will bring us in time to greater knowledge than we have yet attained.

Scientific research and invention, however great the genius which guides it, is largely a matter of perseverance. We have to try systematically the ninety-nine things that do not succeed before we find the one that does.

Hence I conclude my life-story by quoting some appropriate lines which once caught my eye in a newspaper years ago, with apologies to their unknown author, which are as follows :

“KEEP STRIVING : the victors are those who have striven
And fought for the prize that no idler has won.
To the willing and steadfast alone it is given,
And before it is gained there is work to be done.

KEEP HOPING : the clouds hide the sun for a time,
But sooner or later they scatter and flee.
And the path glows like gold to the toilers who climb
To the heights where men look over landscape and sea.

KEEP ONWARD—right on, till the prize is attained ;
Front the future with courage and obstacles fall ;
By those, and those only, the victory is gained
Who keep faith in themselves and in God over all.”

